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ORIGINAL ARTICLES.

RESEARCHES INTO THE ETIOLOGY OF DENGUE.[1]

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The epidemic of dengue which prevailed throughout the State of Texas during the fall of 1885, was unusual in many respects, for example:

1st. In its universality.

2d. In the violence of its symptoms.

3d. In its manifest contagiousness.

4th. In the protracted convalescence of its subjects.

5th. Its hæmorrhagic tendency.

6th. In its numerous sequelæ.

From Galveston, located in the South-eastern portion of the State, it spread, in the course of a few months, to its Northern border. During this time almost every city, and many of the country districts within these limits, fell under the epidemic influence of this disease. In the city of Austin about 16,000 cases of dengue occurred during this time, out of a population of 22,000 inhabitants. I am informed that other cities and sections suffered as severely, *i.e.*, had as many cases in proportion to the population as did Austin.

Many cases where direct contagion was the cause of this disease came under the observation of the writer. He has no doubt that by this means it was carried and spread from one neighborhood to another, and from the city to the country by infected individuals. The violent type of the disease, the protracted convalescence of its victims, and its sequelæ, were unusually prominent in this epidemic. The *first*, by the suddenness of its invasion, often with an initial chill, its pyrexia, its excruciating pains, nausea, and the constancy of its exanthemata. The *second*, by the mental and physical prostration which follows its defervescence; this often continues for weeks or months, with anorexia and indisposition to perform any kind of labor.

The sequelæ most frequently observed were dysentery, entero-colitis, nephritis, bronchial and gastric catarrh, abscess, glandular enlargements and eruptions of the skin. It would seem from the peculiar clinical history of dengue, especially its contagiousness and its epidemic character, that it is a specific disease; that micro-organisms are the source of its infection, and these micro-organisms find in the blood a suitable environment for their growth and multiplication.

To determine the correctness of this theory, and if possible to obtain information with reference to the etiology and pathology of this malady, which is the scourge of our southern country, and especially as medical literature is absolutely silent upon these matters, the following experiments were conducted, with hopes that they would lead to a more rational therapy, or perhaps indicate some method of protecting individuals against dengue by means of inoculating them with the attenuated virus. I found, however, that I had neither the time nor means of carrying out the necessary investigations to determine the practicability of this hope, or of accomplishing these latter anticipations. A physician engaged in active practice has but little time, whatever his inclination may be, to indulge in experimental work. The demands of his patients and his creditors too frequently prevent his giving that time to original work or pure scientific labors which the interests of his profession demand.

During the six months occupied in making these investigations, one-half, and frequently two-thirds, of nights succeeding days of toil, were utilized in these labors; they possessed my interest; I managed to give them much of my time. The work actually performed during this time is embraced in the following statements:

1st. Blood, which was obtained from living subjects during the various stages of dengue, was microscopically examined, (a), directly, that is, without the addition of any chemical reagents; (b), after it had been subjected to the action of certain chemical reagents, viz: glacial acetic acid, with and without dilution; caustic potash in solution, both weak and strong; chloroform, and ether.

2d. This blood was carefully dried upon sterilized cover-glasses by passing these through the flame of a spirit lamp, and then subjected to the action of various staining reagents.

3d. Dengue blood obtained from living subjects was introduced, upon the point of a platinum wire, into test-tubes containing sterilized culture jelly prepared for this purpose.

These tubes were closed with plugs of sterilized cotton, then placed in an incubator, where the temperature was kept at 100° F. for the growth of such organisms as were contained in the blood.

4th. Blood was drawn directly from the veins of

[1] Read in the Section on Practical Medicine at the Thirty-Seventh Annual Meeting of the American Medical Association, St. Louis, May, 1886.

a living subject into a series of sterilized glass bulbs, which were united by a capillary-tube. This was performed in such a manner that it seems impossible for germs from the air, or by other accidental means, to have gained an entrance into these bulbs. These were also kept in the incubator, at a temperature of 100° F.

5th. The matters vomited and urine passed by dengue subjects were subjected to microscopic examinations.

It is the purpose of this paper to record the methods adopted in making these examinations, and the results obtained from them. Before entering into the technique of these methods, which are necessarily tiresome, I think it better to submit the following outline of the results obtained: In the blood examined directly, or after its treatment with the chemical reagents already referred to, stained or unstained, I invariably found, often in great numbers, in the cell elements as well as in the plasma, micrococci about $\frac{1}{250}$ to $\frac{1}{300}$ the diameter of the red cells, spherical in shape, and red or purplish in color; when these were seen in great numbers one layer superimposed upon another; frequently seen in the cultures, they appeared of a black or brownish color, but when seen singly or in thin layers in the blood or in the cultures, the red color is always distinct and characteristic.

During the development of this organism, at some period in its life-history, from causes which I do not understand, it becomes surrounded by a gelatinous envelope; this I have frequently observed in the blood and in the cultures alike. I always succeeded in growing in the culture-tubes, upon the surface of the jelly, micrococci, and no other form of bacteria, which in color, size and behavior, are identical with those seen in the dengue blood. The blood contained in the series of glass bulbs was examined, some after the lapse of six weeks, others at three months. In both instances I found that the blood contained a pure culture of micrococci which, in all respects, were the same as those which I had previously seen in fresh blood, and grown upon culture jelly, in culture tubes; these were apparently composed of micro-organisms held together by their gelatinous envelope, or capsules; at the end of the casts, where the micrococci were less firmly attached, or cemented together, their shape, size and color were found to correspond with those seen in the blood, or grown in the culture-tubes, or bulbs.

The microscopic examination of the blood for micro-organisms is attended with many difficulties. The first care is to obtain blood for examination that is not contaminated with bacteria from the air, or from other outside sources. In all examinations of the blood, or other portions of the body, for micro-organisms, every precaution against its accidental contamination, or the introduction of germs from outside sources, should be taken; without such precautions all investigations of this sort would be worthless and delusive.

When it is remembered that air and water contain large quantities of micro-organisms, that these adhere to the glass apparatus, metal instruments, fingers, and clothing of the operator, and that bacteria will quickly form in distilled water, and in staining fluids required in such examinations, the necessity of thoroughly washing and then sterilizing by heat and by chemicals, when this is practicable, all articles used, and by boiling and filtration, or other efficient means, becomes at once apparent.

The methods which I adopted to secure cleanliness and sterilization of all glass apparatus, instruments, and other agents used in these experiments, may not have been absolutely perfect in every instance. I made them as nearly so as the means at my command would allow. I am constrained to believe, from the uniformity of the results obtained, notwithstanding some minor defects in the detail of my technical methods, that, in the main, these were sufficiently exact to exclude serious errors.

The second difficulty which confronts him who examines blood for schizomycetes, is to dry the blood on the cover-glass without overheating, and thus disintegrating it.

The third difficulty is, to not mistake for micro-organisms certain elements and granules found in normal and pathological blood. These mistakes have occurred frequently, and wrecked many a beautiful hypothesis. That the reader may know and appreciate the difficulties to which I refer, I quote from the recent work of Dr. F. Hüppe:[1] "The examination of blood for bacteria offers very great difficulty, because in the normal blood within the vessels, and in the normal disintegration of the healthy blood, granular elements are present, or are formed, which under certain pathological conditions, in anæmic state and in fever, are increased in number, and can be easily mistaken for micrococci.

"They have already been often confounded with micrococci, and are almost daily mistaken for them, *e.g.*, the renowned syphilitic corpuscle, and the so-called organisms of the venom of serpents. Here belongs also much of what has been spoken of as the development of bacteria from nitrogen molecules, from microzymen, or from the anamorphosis of protoplasm. An exact study of these granules of the blood is, on this account, an indispensable desideratum in bacteria investigation.

"These granular forms further constituent parts of the cellular elements of the blood, and on this account again are of interest in ætiology, because there are parasites which are similar to the amœboid cells, *e.g.*, those monads found by Lewis in the blood of rats, by Koch in the blood of marmots. The elements of the blood, which directly or through their granules may be confounded with micro-organisms (with the exception of the red blood corpuscles), and the products of their disintegration), are divided according to Ehrlich into

"1st. Lymphoid elementis, (a), small lymph cells, (b), large lymph cells.

"2d. Myeloid cells (eosinophile).

"3d. Undetermined spleen and [or] marrow: (a), large mononuclear cells, (b), transitional forms, (c), polynuclear.

"The small lymphoid elements are somewhat

[1] "Methods of Bacteriological Investigation," by Dr. Ferdinand Hüppe. D. Appleton & Co., New York. 1886.

smaller than the red blood corpuscles, possess a very large nucleus, so that there is very little or no protoplasm to be seen.

"The large lymph elements are a further development of the first, and are only to be differentiated from them in this, that they possess, around the large nuclei, a distinct border of protoplasm.

"The myeloid elements are large round cells, with large oblong nuclei.

"The mononuclear cells are about three times the size of the red blood corpuscles, possess round or oval nuclei of large size, and a considerable mass of protoplasm. The mononuclear transitional forms are to be differentiated from these cells only in this, that the nuclei are no longer round or oval, but have become indented.

"The polynuclear elements are somewhat smaller, but still are always larger than the red blood corpuscles, and their nuclei show, as a further differentiation, a polynuclear form; these are the true white blood corpuscles.

"The granular elements, or granules, which are present in the cells, and which become free in the destruction of the same, are divided with respect to their reaction to aniline dyes.

"The α or eosinophile granule is coarsely spherical, strongly refracting, and can be stained in all the acid aniline dies. It is present in the myeloid elements, seldom in the normal blood, and its number is greatly increased in leucæmic processes.

"The β, or amphophile, is formed especially in the marrow, very often in the leucocytes in the blood of rabbits and guinea-pigs, and can be stained by acid and basic aniline dyes.

"The γ, or basophile plasma-cell granule, can be stained, like the bacteria, by the basic aniline dyes. These granules are coarse, slightly refracting, almost completely wanting in normal human blood, increased in leucæmic processes, and are present normally in the blood of the lower animals, especially the white rat.

"The δ, or basophile granule, is fine, and can be stained in basic aniline dyes, and forms a constituent part of the large mononuclear elements.

"The ϵ, or neutrophile granule, is very fine, and fills the polynuclear elements of the human blood quite thickly, is present sparsely in the transitional forms, and very seldom in the mononuclear elements. It can be stained by the neutral dyes. Without recourse to staining, these granules, as a whole, and also the products of the disintegration of the red blood corpuscles, may be confounded with micrococci.

"By systematic staining with aniline dyes the α, β and ϵ granules can be excluded. An error is then possible only with the γ and δ granules, because these are stained in the basic aniline dyes, the same as the bacteria. These last, on account of their fine grain, can, with comparative ease, be differentiated from micrococci, and have not, as yet, been confounded with them.

"The plasma-cell granules, on account of their medium size, come so near to the known forms of cocci that not only the individual free granules in the blood have been considered as cocci, but even the so-called plasma-cells in the tissues have been described as colonies of cocci. They can, on purely morphological grounds, be differentiated from them in this way, viz: That they do not all have the symmetrical appearance of the cocci, but present the greatest differences in the size of the granules.

"If it be desired to examine the blood for bacteria, a small drop is rapidly spread out in a thin layer, dried, fixed, and then passed three times through the flame, and then stained in the ordinary manner. In such preparations the bacteria are sufficiently stained, but not the granules."

Prolonged heating of cover-glass preparations, or sections containing these granules, is necessary to their successful staining. The method of doing this, which is recommended, is to subject the specimens, in a drying oven, to a temperature of 240° F. for an hour; they are then prepared for the staining process, and may be stained with their respective dyes, acid, basic or neutral, in the manner described.

As micrococci and other forms of schizomycetes do not require this prolonged heating in order to their successful staining with the aniline dyes, renders this difference in the behavior of the protoplasmic granules on the one hand, and micrococci on the other, an efficient means for their differentiation.

All the glass apparatus and instruments used in the following examinations of dengue blood were sterilized by the following methods: The cover-glass and microscope slides, which had never been previously used, were first washed in warm water with soap, then in clear water, then immersed in Seiler's cleansing fluid, prepared as follows:

Bichromate potash	2 oz.
Sulphuric acid	3 fld oz.
Water	25 fld oz.

When they were taken from this solution, they were again washed in clear water, and finally heated in the flame of a spirit lamp just before they were used. The metal instruments were washed in warm water and soap, clear water, and then in a 5 per cent. solution of carbolic acid; they were also heated in the flame of the lamp just before they were used. The platinum wire was fused to the end of a glass rod, this wire was sterilized always before using, by heating it to whiteness in the lamp. The arm or finger of the person from whom the blood was obtained was washed with warm water and soap, then in a 5 per cent. solution of carbolic acid.

The part was then punctured with a sterilized needle, and a small portion of the blood which appeared at the point of puncture was transferred by means of the sterilized platinum wire to the sterilized cover-glass, this was covered by another glass in order to obtain a thin layer of blood upon each of them; they were then separated, dried, and finally passed three times through the flame of the lamp, in order to thoroughly coagulate the albumen, and fix the blood elements.

Blood for examination was obtained from about forty typical cases of dengue, at various times and places, and during the various stages of the disease. The results which I obtained from examinations of these different specimens were entirely uniform.

Dengue blood dried upon the cover-glasses in the manner described was subjected to the action of certain chemical reagents, and aniline dyes, with the following results, viz : some were treated with a 10 per cent. solution of caustic potash, others with glacial acetic acid, and others with ether and chloroform. None of these chemical agents had any destructive effect upon the micrococci which the blood contained—they could be seen as brilliantly and distinctly after treatment as before.

A peculiar effect was produced upon the microorganism by the glacial acetic acid. This agent stained these organisms brownish, and changed their shape from spheres to ovoids; these were seen in such numbers that the field of the microscope was covered by them. I think this change in shape was only apparent, and was caused by the action of the acid upon the capsules of the cocci. I am sustained in this opinion by the fact that the organisms seem larger after treatment with the acid; this was due to the invisible capsule being made visible by this agent. Inasmuch as there are no tissue elements in normal or pathological blood, as corpuscles, granules, cells or cell elements, which can resist the destructive action of each of these chemical reagents, whilst bacteria, on the other hand, can resist them, it would seem evident that if spherical bodies, uniform in size, shape, color and behavior, are found in the blood in certain pathological conditions, and these bodies can resist the destructive action of these chemicals, they must be micrococci, and cannot possibly be anything else. The remainder of these cover-glass preparations were subjected to the action of the various aniline dyes, in both weak and strong aqueous solutions, in aniline oil, water, and in alkaline solution.

The results which were obtained by these methods of staining were very instructive and important; for instance, it was shown:

First. That the dengue micrococci do not stain with the aniline dyes as readily as do other forms of bacteria.

Second. Methyl aniline blue in a weak solution of caustic potash furnishes a staining fluid for which the cocci of dengue manifest an especial affinity.

In order that the importance of this matter may be fully appreciated, I shall quote the following remarks from Friedländer:[1] "Furthermore, if among a certain number of elements which appear to be identical, some conduct themselves in a peculiar way toward a certain reagent, while others do not—if, for example, some are stained by a certain dye, while others remain colorless, we must necessarily conclude that there was a primary difference among the elements. Upon this principle are formed all the methods of preparation, and some of them very complicated, which are employed for the exhibition of the different histological elements. The principle in staining is, therefore, that certain elements or tissues, and also of cells, appropriate actively, or in large quantity, from the solution employed, a certain dye, and form with this a combination having an intense color, that is more or less permanent. . . . Hence in many instances staining assumes the importance of a chemical reaction, by means of which any particular structure that lies concealed among other bodies, can be brought easily into prominence.

"This 'elective' action of dyes is of extreme importance in pathological investigations. . . . The technique of dyeing is usually this: A section is transferred from distilled water to a dish filled with the staining solution, with which it is entirely covered; it remains in this for different lengths of time, varying from a few minutes to twenty-four hours, and is again immersed in distilled water, in order to wash away the portions of the dye that are adherent to its exterior.

"In many cases, however, the section which has been removed from the staining solution, and washed, is subjected to further manipulation; it is again decolorized, that is, partially. In this instance there has occurred at first a diffuse, even, but unnecessary amount of staining; but during the supplementary process of extraction, while certain elements give up their staining completely, others, that have a stronger affinity for the dye, retain it. This is called by Ehrlich the principle of maximum staining. In accordance with these principles of staining, the blood, which had been dried upon the sterilized cover-glasses, was submitted to the action of various aniline dyes, to find, if possible, some staining fluid with which isolated staining of the micrococci could be produced."

With this object in view, I tried successively Bismark brown, vesuvin, gentian, violet, methyl-violet, fuchsin, methyl-blue, aniline-green, picro-carmine, and eosin in watery solutions, and in solution with aniline waters. This result was obtained imperfectly with the solution of fuchsin in aniline water, and perfectly with methyl-blue in a solution of caustic potash.

With all the other dyes named, the results were negative, *e. g.*, all parts of the picture were stained alike—cells and organisms—and they were all decolorized with equal facility when washed in a 1 per cent. solution of acetic acid and then in absolute alcohol. With the methyl-blue potash solution, however, a very different result was obtained; this dye, in the solution referred to, manifested such an affinity, or elective action for the organisms of dengue, that these would retain the blue color after this had been extracted from the blood cells by the decolorizing agents named.

The manner of preparing this solution, and the method of staining with it, which were adopted, are as follows: Concentrated alcoholic solution of methyl blue, 30 c.cm.; solution of caustic potash, 1 to 10,000, 100 c.cm. In a dish filled with this fluid the cover-glasses were floated, with their blood sides downward. The dish was then covered to exclude dust, and the cover-glasses were kept in this condition from twelve to twenty-four hours. Better results are obtained by keeping the staining fluid during this time at the temperature of 100° F. The cover-glasses are then removed from the staining solution and washed in the 1 per cent. solution of acetic acid, then in absolute alcohol, until the color is entirely or sufficiently removed.

[1] The Use of the Microscope in Clinical and Pathological Examinations. By Dr. Carl Friedländer. D. Appleton & Co. 1880.

I think a better picture is obtained, and the relative position of the organisms to the cells shown, if the process of extraction is arrested before the cells are entirely decolorized; they should then be mounted in Canada balsam, and examined with a high power, and with a large diaphram or open condensor. The blood cells should show of a faint blue color, whilst the micrococci, which are to be seen in the blood cells and plasma, will be stained an intense blue. The inability of these organisms to hold the other aniline dyes, acid or basic, to which they were exposed, their uniform size, their presence in the blood cells, their ability to resist the destructive action of acids, alkalies, ether, etc., it would seem are sufficiently distinctive to differentiate them from protoplasmic granules, or the products of cell disintegration. An additional reason for regarding them as micro-organisms exists in the fact that they can and have been grown upon culture media, outside of the body.

In conducting these culture investigations, the same care to guard against error in results was practised as in the former. The test tubes, which had not previously been used for any purpose, were thoroughly washed with soap and water, then in clear water, dried, and plugged with absorbent cotton; they were then put into a furnace and exposed to a temperature of 400° F. for an hour. The culture medium which was used in all these investigations was that known as Miguel's lichen jelly; this was used in preference to gelatine, blood serum or other solid culture media for the following reasons:

It melts only between 55° and 60° C. (131° to 140° F.), which permits of the cultivation of such organisms as require for their development elevated temperatures. Ordinary nutritive gelatine melts before 30° C. (86° F.). Second. It remains without alteration or losing its power of solidifying when exposed to a temperature of 100° C. (230° F.) for rigerous sterilization. Gelatine, on the contrary, is reduced under such conditions to a turbid broth, which remains fluid on cooling. It is prepared by digesting Irish moss—*crispus chondrus*—in beef broth.

The broth is prepared as follows: Allow sixteen ounces of lean beef, as fresh as possible, two quarts of water, and sixteen grs. of table salt, to simmer for four hours in an uncovered vessel, then close it and boil for an hour, cool, and skim off the fat, neutralize carefully with carbonate of soda and filter.

In preparing the jelly, one ounce of Irish moss was digested in one quart of the broth, at a temperature short of boiling, the resulting decoction was passed through a colander to separate the swollen leaves, and then filtered through sterilized bolting cloth. It was then boiled for an hour, and whilst at this temperature the test-tubes were filled with this jelly, by means of a sterilized pipette, to about half their capacity; cotton stoppers were then introduced in the tubes, and these were subjected to a temperature of 212° F. in a Koch and Grafiky sterilizing cylinder for an hour each day for seven successive days.

After the last heating, the tubes were placed at an angle whilst the jelly cooled, in order that as large a surface as possible should be presented upon which the organisms could grow and multiply. Many test tubes were sterilized and filled with the jelly, and subsequently subjected daily to the temperature of boiling water in the manner above described. These were carried to the homes of those persons from whom dengue blood was obtained for inoculation purposes.

In obtaining this blood, every possible danger of introducing germs by accidental means was guarded against. The method of washing the arm and sterilizing the needle with which the puncture was made, has been described. A small wire of platinum, fused to the end of a glass rod, was used to convey a small quantity of the blood which appeared at the point of puncture into the test-tubes, where the point was brought in contact with the surface of the jelly. The only chance of introducing air germs occurred during the short time the cotton plug was removed to allow the passage of the platinum wire; the wire, of course, was heated to whiteness just before its use. Some twenty tubes of jelly were directly inoculated with the blood of dengue subjects; the blood used was obtained from different individuals, and always at their homes. These tubes were then put into an incubator, where the temperature was maintained constantly at 100° F., for the growth of the dengue organisms.

Without a single exception every tube which had been inoculated in the manner described showed, upon the surface of the jelly at the point of inoculation, a white spot elevated above the surface of the jelly; this gradually enlarged and could be seen, faintly outlined in the jelly, at the expiration of twenty-four hours. When this growth was examined under the microscope, with a high power, it invariably showed a pure culture of micrococci—which in color, size, shape and behavior, were identical with those seen in the blood of dengue subjects.

The uniformity of these results, in all the tubes inoculated, in growing these micrococci, and never other forms of bacteria, would certainly indicate that the matters of inoculation came from a common source, *e. g.*, the blood. If they had been accidentally inoculated from the air, or by the platinum wire, or through want of absolute sterilization of the tubes, many other forms of bacteria and fungi would have been found growing on the jelly, and these tubes, when opened, would have had the odor of putrefaction, which was not present in any of them. These cultures were continued from test-tube to test-tube for a period of six months. The method of perpetuating these pure cultures of the dengue micrococci, was to inoculate in the manner described, the sterilized jelly, in a fresh tube, from one which had previously been inoculated from the blood, then a third tube from the second, a fourth from the third, etc., through many generations of the organisms.

When it is considered that these culture investigations occupied a period of six months, and that during this time many transfers occurred from test-tubes containing micrococci to those which did not contain them; that each transfer removed by generations the organisms from those contained in preceding tubes; that frequent microscopic examinations of the differ-

ent series of tubes invariably revealed a pure culture, *e. g.*, without any other forms of bacteria; it would seem that the methods of sterilization employed had been successful in excluding alien germs, and that those found had a common origin from dengue blood.

The life history of these organisms was watched daily, and studied by means of Holman's life-slide. In the old cultures, those which were removed by many generations from the blood, the organisms were smaller, less deeply colored, and less frequently encapsulated, than those seen in the blood, or removed from it by a few generations. When examined in water without being stained, or in other fluid media which possess a low index of refraction, the micrococci are seen in active movement (Brown movement).

In those cultures which are removed by only a few generations from the blood, the cocci are seen surrounded by capsules. These are sometimes faintly colored pinkish, and can be distinguished from the cocci, which possess a deeper color.

Two methods of generation were observed: The first by fission; the organisms are seen to divide through their centres; each coccus will thus form two others. Frequently these newly formed organisms remain united together so as to assume the form of a rod or chaplet; this indicates that they belong to the class named streptococci. In other cases they unite to form swarms or zoogloæ. These latter become more compact, the distinctions of the cocci less, the color of the mass deeper, until they finally contract into corpuscular bodies, about the size of red blood cells; these often unite to form filaments, which frequently assume the shape of a network. This I have often seen when the culture was thinned with distilled water and allowed to dry upon a microscopic slide. This arrangement can be seen, whether the specimens are stained or not. The swarms and chaplets of the micrococci can be much better seen in the stained preparations. A histological stand, Abbé's illuminating apparatus and $\frac{1}{12}$ H. I. objective, manufactured by J. Grunow, of New York, were used in these researches.

The results obtained in the following and last series of investigations of dengue blood are to me very satisfactory and conclusive, inasmuch as the means adopted to exclude alien germs were, as far as I can see, absolutely perfect.

The apparatus used consisted of a series of glass bulbs united and blown upon a capillary glass tube—Liebig's potash bulbs. To one end of this apparatus was attached a new hypodermic needle, by means of a short piece of new rubber tubing. To the other end of the glass tube (which was packed with cotton) was attached an aspirator.

The following is the method used in sterilizing and using this apparatus, viz.: The series of bulbs were first chemically cleaned, one end of the tube was then packed with absorbent cotton, and the bulb heated in a furnace for an hour at 400° F. The rubber tubing, with the hypodermic needle attached, was treated with a 20 per cent. solution of carbolic acid, washed in clear water, and exposed to the temperature of escaping steam for an hour, in the sterilizing cylinder. It was then dried in the furnace, the free end of the tubing slipped over the free end of the glass tube, and the end to which the hypodermic needle was attached was encased with a sterilized test-tube, this was held in its place and the needle protected from air germs by means of absorbent cotton, that was packed firmly in the mouth of the test tube, and around the needle. The entire apparatus as thus arranged, tubing and needle attached, and covered with test-tube, was again put into the furnace and heated to 300° F. It was then removed from the furnace and carried to the home of the gentleman who kindly donated a sufficient amount of his blood for this investigation. He had a typical case of dengue. His arm was washed with soap and warm water, dried with a freshly ironed towel, and then a solution of carbolic acid, sufficiently strong to quickly whiten the skin, was applied to that part of the arm where the puncture was to be made. A ligature was then applied to the arm above the elbow. The test tube was removed from the hypodermic needle, and this was quickly passed into the large vein, at the bend of the arm. An aspirator was then attached to the free end of the glass tube—which was packed with sterilized cotton—and the bulbs partially filled with blood by means of aspiration. The glass tube, next to the rubber, was then closed and separated by the blow-pipe, before the needle was withdrawn from the arm.

These bulbs containing the dengue blood, which were effectually closed against the admission of all germs, and which contained no germs except those which entered with the blood from the veins of the donor, were then put into an incubator at 100° F. temperature, in order that a pure culture of the dengue micrococci might be grown in the bulbs, with their contained blood as the nutritive medium.

The first bulb was removed with the blow-pipe, and examined at the expiration of six weeks. The blood was examined, first, directly, without admixture; second, after treatment with glacial acetic acid, undiluted and in weak solutions; third, with 10 per cent. solution of caustic potash and in weak solutions of this alkali; and fourth, finally subjected to the various aniline dyes, before referred to.

In the direct examinations, a small drop of blood was placed upon the cover-glass, this was then inverted upon a slide, so as to obtain for examination a thin layer of blood; or the blood was examined in ⅔ per cent. salt solution or a solution of osmic acid 1 to 300. In those specimens examined directly, the field appeared to be covered with blood cells and dark pigment granules; these latter were seen in immense numbers.

When the blood was examined in the salt solution, or in the solution of osmic acid—this latter fixed the cells, and by giving them a faint brown color, rendered them more distinct—it was seen that what appeared as dark pigment granules in the direct examination, was now recognized as dengue micro-organisms, with all their distinctive features. The red cells in many cases were absolutely crowded with these, whilst in the plasma they were seen free, in swarms, zoögloea, masses, and in corpuscular bodies, which sometimes united to form filaments, as in the

jelly culture. Cover-glass preparations of the blood, from this culture bulb, were subjected to the various aniline stains.

The indisposition to hold any of these dyes, except the methyl-blue potash, was as distinctly manifested as in the fresh dengue blood. The only difference in the appearance of the micrococci as observed in the fresh blood and that obtained from the culture bulbs is this: The organisms, when stained, appear larger in the latter than they did in the former; this, I think, is due to the fact that in the culture bulbs they are more frequently encapsulated. When both cocci and capsule are stained, the size is apparently larger.

The remaining bulbs were examined six weeks later. They all contained pure cultures of the dengue streptococcus, with disintegrated blood cells. These organisms were seen in zoöglœa, swarms, and in compact corpuscular masses; these were often united to form filaments. Normally shaped blood cells were found in only one of these last series of bulbs. I think their preservation in this bulb was due to a layer of coagulated serum which had formed over the surface of its contained blood. Many of the blood corpuscles in this bulb were more or less disintegrated, those which retained their shape were clouded by coagulation of the cell protoplasm, which concealed the organisms they contained to a greater or less extent. These, however, could be seen in great numbers, when the blood was examined in a weak alkaline or acid solution, which rendered the cells more transparent. None of the bulbs, when opened, had the least odor of putrefaction, and the only micro-organisms which they contained were those peculiar to dengue.

Pure cultures of these were found in each of the series of bulbs, and were seen in their various stages of development, *e. g.*, with and without capsules, in swarms, in zoöglœa masses and in corpuscular shaped bodies, frequently united to form variously shaped filaments. In these filaments the corpuscular masses, and often the cocci which formed them, could be seen faintly outlined.

This ends, at least for the present, these investigations. No one better knows than myself the incompleteness of this work, nor appreciates better the possibilities which might have resulted from its continued prosecution. The want of time and facilities for its proper execution prevented me from undertaking any additional labors in this matter. The investigations were undertaken without bias, and the conclusions arrived at are the result of careful, conscientious work. Whether these conclusions are true or false must be decided by the future.

I am greatly indebted to Dr. R. Munger, of San Antonio, Texas, for excellent micro-photographs of dengue blood, obtained from the first culture bulb. The zeal and patience displayed by him in this work are characteristic of the lover of science, while the excellent results obtained mark him as an expert in these matters. Many efforts were made by him to photograph the specimens colored with methyl-blue; success was attained only with those specimens which were stained after Sternberg's method for photographing micro-organisms, viz.:

A drop of sulphuric acid was placed upon the blood dried upon the cover-glass; this was allowed to remain for a minute, and then washed off with distilled water. The cover-glass was then floated, blood side downward, upon the following solution, in a watch crystal, viz.:

Iodine	grms.	3.
Iodide of potassium	"	5.
Distilled water	"	500.

After a few minutes' exposure the preparation will be found to present a deep orange color, which gives the desired contrast in a photographic negative. The micro-photographs which he obtained from specimens stained by this method displayed the blood cells, in which appeared many micrococci, while zoöglœa and swarms are seen in the blood plasma.

In conclusion, I desire to express the regrets I feel in not being able to complete these investigations. Zopf, Koch and others indicate among the questions to be answered in a thorough study of any bacteria, the following:

1. Shape, size, color and details of structure, *e. g.*, flagella, peculiar envelope, etc. Character and speed of movements.

2. Character of natural habitat. Artificial media best adapted to growth and reproduction. Stages of development passed through. Formation of zoöglœa spores, filaments, rods, cocci, "swarms." Conditions under which such formation occurs. Character of colonies formed in firm culture media.

3. Capability of producing fermentation, putrefaction. Character of decomposition products, volatile and other, formed in various nourishing media.

4. Behavior toward oxygen at normal and altered pressures. Behavior toward other gases.

5. Effects of various temperatures on movements, germination, etc.

6. Behavior in relation to light (phototonic properties). Behavior toward electricity.

7. Behavior toward antiseptics and poisons.

8. Are the forms under investigation found in a diseased organ or tissue. What is the effect of inoculating animals of different orders and species with pure cultures. If *virulent*, can the *virulence* be attenuated by exposure to air, to antiseptics, to heat, or by repeated "fractional" cultures. Under what conditions. Does the inoculation of attenuated germs have a *cumulative* effect if repeated at short intervals. Does one inoculation give immunity towards a second made with virulent microbes.

It will be seen that the character of investigation indicated by these questions could only be carried out by persons possessed of an abundance of time and money, and a willingness to devote both to such labors, or under the auspices and at the expense of the State or Federal Government. In view of the brilliant results already attained by such investigations, *i. e.*, the protection furnished, by inoculation with the attenuated microbes, against the following diseases, viz.: small-pox, hydrophobia, anthrax, yellow fever,(?) hog typhus and chicken cholera, it would seem a wise, humane and economical policy on the part of the State or Federal Government to encourage, by money appropriation, such investigations. Among the proba-

bilities which such investigations promise may be reckoned the prevention of many, perhaps all, the epidemic and contagious diseases to which man is heir.

Pasteur has astonished the world with his protective inoculations. His latest and most wonderful achievement is the immunity from hydrophobia which he claims is secured by inoculations with the attenuated virus.

Not less remarkable are the reputed discoveries of the yellow fever cryptococcus by Dr. Freire, of Rio Janeiro, and Dr. Carmona, of Mexico, and the immunity secured by inoculation with these organisms, as claimed by these gentlemen.

Other diseases and their specific microbes, notably diphtheria and phthisis, are under investigation by experienced bacteriologists, and in this day of marvels, we may expect soon to hear that these also are brought under the controlling influence of vaccination.

In the light of this experience, I feel secure in prophesying that within the next decade, dengue and many other contagious and infectious diseases will be as certainly prevented by inoculation as small-pox now is. It remains to be seen what contribution of knowledge will be furnished by America to this promising system of preventive medicine.

MANAGEMENT OF THE SECUNDINES.[1]

BY WM. H. WATHEN, M.D.,

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One year ago, at the meeting of the American Medical Association in New Orleans, I read a paper in this Section on the "Treatment of the Membranes in Abortion and in Labor," but I do not offer an apology for reading another to-day.

Authorities in medicine do not agree as to the management of the third stage of abortion or of labor, and the subject is being extensively discussed in this country and in Europe. At the meeting of the British Medical Association at Belfast, in 1885, it attracted the attention of distinguished physicians of England, who failed to agree in many respects as to the best mode of treatment.

Felsenreich, in the January, 1886, number of the *Wiener Klinik*, and the great French obstetrician, Professor Pajot, in the February number of the *Archives de Tocologie*, wrote exhaustive papers on this subject, the former advocating and the latter opposing expectation. Most physicians who leave the expulsion of the membranes to nature are willing to remove the placenta when it is expelled from the uterus, but there are some who refuse to do so when it lies loose in the vagina. There are many who believe expectation is pernicious, and who remove the membranes from the uterus by expression, after the Credé or Dublin fashion, or draw upon the cord or edge of the placenta; or if the membranes adhere introduce the fingers or a hand into the uterus and separate and remove them.

[1] Read in the Section on Obstetrics of the American Medical Association, at St. Louis, May 4, 1886.

Nor is our profession agreed as to the mechanism of the separation of the membranes from the uterus. It is argued that it may result from contraction or reduction of the placental area of the uterus, causing a retro-placental clot at the centre or at the periphery of the placenta, which completes the separation; or from a relative inequality in the contractile or retractile powers of the placenta and membranes, causing a separation in the trabecular structures connecting the decidua serotina and decidua vera to the uterine musculature. Neither is there a settled conviction as to whether the separation of the membranes occurs during or after the expulsion of the child. In an excellent paper on the "Anatomy and Relations of the Uterus," etc., in the *Edinburgh Medical Journal* of 1884, by Dr. Freeland Barbour, and from the investigations of Dr. Engelmann and others, it appears conclusive that, when the placenta is well formed, the musculature of the uterus is united to a compact layer of the decidua vera, decidua reflexa, decidua serotina, and the chorion, by a spongy or trellis-like arrangement of enlarged uterine glands and connective tissue.

The chorion and the amnion are seldom firmly united, though the union becomes more intimate toward the end of pregnancy, but at term the amnion may sometimes be easily dissected from the placenta. In the separation of the membranes the fundi of the glands with ciliated epithelium remain attached to the uterus and develop into new mucous membrane.

The formation of the placenta begins about the end of the second month, and it increases in size until the end of pregnancy, but in the latter months the union between the placenta and uterus becomes less intimate. Until the formation of the placenta the embryo is surrounded by the amnion, the chorion, the decidua reflexa, decidua vera, and decidua serotina. But the decidua vera and the decidua reflexa do not unite for several months after conception, and the amnion and the chorion are not in direct contact until between the end of the third and the end of the fifth month. It is important to remember this, for the fœtus may be expelled in an unbroken sac at any time before the sixth month—possibly in the sixth month—the placenta and other membranes being retained. In these cases the amnion has not reached the placenta, or is so loosely attached that it separates easily, the umbilical vessels being torn between the amnion and the placenta without disturbing the integrity of the amniotic sac.

We will divide the subject into the treatment of the third stage in abortions before the beginning of the third month; then the treatment of the third stage in abortions from the end of the second month to the end of the seventh; and finally, the treatment of the third stage in premature labor and in labor at term.

Nature separates and expels the membranes by contraction and retraction, the placenta presenting at the os and coming away edgeways. This fact can be demonstrated in nearly every case of labor by grasping the placenta in the hand as it is forced through the neck of the uterus. It is possible that in fundal implantations of the placenta, it may some-

times present by its fœtal or amniotic surface with its long diameter in the os; or pulling on the cord may so invert the placenta as to cause the same abnormality.

Hæmorrhage in the third stage of labor at any period of pregnancy is controlled by physiological contraction and retraction, and by the formation of trombi, or fibrinous coagula in the torn ends of the utero-placental vessels, which is favored by contraction of the muscular fasciculi, which surround the vessels and contract their caliber.

In abortion after the eighth week, in premature labor, and in labor, the membranes should be removed, whether separated or adherent, when they are not expelled within twenty to thirty minutes, and they should be removed sooner if we are not apprehensive of hæmorrhage, retention of the placenta, or of irregular contractions of the uterus. If, in abortion, the woman shows symptoms of exhaustion or syncope, and bleeding continues, the shock in removing the membranes is not greater than that of tamponing the vagina.

In the statistics of Dr. Weir, of Copenhagen, expectation was followed by the retention of the decidual membranes in 1.78 per cent. of cases, while in expression the per cent. was 2.3. But in all other respects the results favor the immediate removal. Post-partum hæmorrhage occurred in expectation in 5.78 per cent. of cases, and in expression the per cent. was 2.3. In expectation manual removal of the placenta was necessary in 1.33 per cent of cases, and in expression in .64 per cent. Secondary hæmorrhage followed expectation in 77 per cent. of cases, and expression in 32 per cent.

Mr. T. M. Watt (Hovingham) in a large experience has seen no cases of post-partum hæmorrhage except in patients treated by expectation.

James P. Nevin, of Ballymoney, in over 200 midwifery cases saw but one case of post-partum hæmorrhage; that occurred in a patient where a midwife had allowed the placenta to be retained three hours; the woman died.

Prof. Pajot's statistics show that in 68 cases of retained placenta, which were left to nature, sixty resulted fatally, and in 102 similar cases, where the placenta was removed artificially and timely, only four died, though some of them were in extreme exhaustion from hæmorrhage when the operation was done.

So long as a retained placenta is in the uterus or vagina the life of the woman is in jeopardy, and she may at any time be attacked with profuse hæmorrhage, septicæmia, and pelvic cellular or peritoneal inflammation. When she has apparently recovered a placental or fibrinous polypus may form in the uterus, or she may suffer from subinvolution, hyperplasia, etc. Several women in Louisville have died within a few years from septicæmia, with pelvic peritoneal and cellular inflammation, or hæmorrhage, caused by a retained placenta.

It may be urged that puerperal septicæmia is always exogenetic in its origin, but we know that a decomposed retained placenta is a prolific cause of the disease, and that its removal or disinfection is the only rational treatment. In abortions before the end of the second month, if hæmorrhage ceases, no effort should be made to remove the membranes, unless they protrude into the vagina and can be taken away without introducing the fingers or instruments into the uterus. These little membranes are generally innocuous, and will be separated and expelled without causing dangerous complications. But if pregnancy has continued until a placenta has formed, expectation should not be practiced. If in abortions after the second month the placenta is not expelled in twenty or thirty minutes, it should be removed, unless the woman is threatened with collapse or syncope from hæmorrhage, and when, from the absence of arterial pressure, hæmorrhage has stopped. We may then wait until she has recovered from shock, or until there is decomposition of the membranes, or a recurrence of hæmorrhage.

If the operation is done without delay the os will usually be dilated or dilatable, and a finger or fingers may be easily introduced into the uterus. There is no instrument that can be substituted for the fingers, though it may sometimes be necessary to use other means to dilate the os. Tents should, if possible, be avoided, and if the os cannot be dilated with the fingers, Ellinger's dilator, or my modification of Leanard's dilator, or Molesworth's dilator may be used. The operation is seldom difficult, and with the patient anæsthetized, any part of, or the entire hand, may be introduced into the vagina, enabling us to examine all the uterine cavity with the fingers and to remove every part of the placenta and membrane. Hæmorrhage will then stop, and there will probably be no other untoward symptom. Of course our hands should be thoroughly disinfected, but this should be done in every case of delivery. In premature labor and in labor at term, the placenta is more easily separated than in the earlier months, and is less frequently retained. I fail to recognize a single fact to justify expectation in the management of the third stage of labor in the latter months of pregnancy, and while I do not believe it usually necessary to supplement or supplant nature in an effort to remove the membranes immediately after the child is born, I do not think the placenta should be left in the uterus more than twenty to thirty minutes, and it should be removed from the vagina immediately.

The membrane can generally be removed by judicious expression during labor pains, but if this fail we may assist expression by introducing some fingers into the vagina and gently drawing upon the end of the folded placenta. With a reasonable degree of care this treatment would neither cause septicæmia nor invert the uterus, and such accidents could only result from criminal ignorance or carelessness in the physician. Unless uterine inertia follows the birth of the child there is no necessity for attempting expression until the uterus contracts in an effort to expel the placenta. We should then follow the Credé method, being careful to express only during a contraction. But it is always safe treatment to keep a hand over the uterus to see that it does not relax, and to encourage it to contract by kneading, massage, or expression, if it fail to do so otherwise.

Credé reports that he removes the membranes in four and one-half minutes after the child is borne with universally gratifying results, and Garrigues has also removed them in from ten to twenty minutes in 400 or 500 cases with excellent results. But it is better to give more time for the membranes to separate and for coagula to form in the mouths of the vessels. A little delay will do no harm, and will be less frequently followed by retention of decidual shreds. If the placenta cannot be separated by expression then it should be separated carefully by a hand introduced into the uterus. If the membranes are imprisoned in the uterus by contraction of the circular fibers of its lower segment or in its entirety, the neck should be dilated with the fingers, and the placenta separated and removed. We should always have a hypodermic syringe charged with ergot for any emergency, but I no not believe that ergot should be given until the membranes are expelled.

DISCUSSION.

DR. FULLER, of Maine, after an experience of thirty-eight years, was in favor of prompt placental delivery, and congratulated the author of the paper upon his method of managing the secundines, and hoped that he would continue in his opposition to expectation. He is usually able to express the placenta in cases of labor at term within five minutes after the expulsion of the child.

In abortions the membranes can not be gotten away so readily, but they should be removed as soon as possible.

DR. WILLIS P. KING, of Sedalia, Missouri, had seen 719 cases of labor at term and an unusually large number of abortions. He removed the placenta promptly both in labor and in abortion; at term by Credé's method of expression, and in abortion, if the os is contracted, he practiced rapid dilatation with the finger or steel dilators. When the finger, hand, or instruments were introduced into the uterus, and in lacerations of the genital tract, he used hot intrauterine injections of a solution of corrosive sublimate (1:4000). He had not seen a case of septic infection for several years.

DR. C. R. REED, of Middleport, Ohio, believed in immediate delivery of the placenta. In nineteen cases out of twenty he was able to deliver the placenta by Credé's method and traction within five minutes after the expulsion of the child.

DR. W. W. POTTER, of Buffalo, New York, said that the terms uterine massage and Credé's method were sometimes employed as synonyms. Suprapubic pressure was not Credé's method. Credé, in 1853, said that your fingers must be forced behind the *corpus uteri*, the thumb over the anterior wall, and the placenta must be expressed just as the stone of a cherry is pinched out. He did not believe in traction on the cord, but when the traction is slight no harm usually results. He called attention to numerous sequelæ of abortion, and to the importance of effecting early evacuation of the *cavum uteri*.

DR. JOHN MORRIS, of Baltimore, advised the early removal of the placenta. He never allowed the placenta to remain longer than twenty minutes after the birth of the child. He distinguished between abortions the result of natural processes, and those induced by medicines and instruments. In induced abortions, it is necessary to practice rigid antisepsis, and the prognosis is less favorable. In so-called "natural" abortion antiseptics are seldom required, and intrauterine irrigation is certainly not indicated. He never gave ergot before the uterine cavity was emptied except in cases of "bleeders."

DR. FRENCH, of Minneapolis, desired to enter a protest against the indiscriminate use of intrauterine irrigation. He had recently observed a fatal termination of a case in which the uterine cavity had been irrigated. He now swabs out the *cavum uteri* when indicated with a mixture of iodoform, carbolic acid, and glycerine. He had seen bad results follow vaginal irrigation with a corrosive sublimate solution, (1:2000). It was necessary to employ bichloride of mercury with extreme caution—if at all.

CORNEAL ULCERS.[1]

BY J. W. THOMPSON, M.D.,
OF ST. PAUL, MINN.

I shall occupy as little time as possible with that material relating to corneal ulcers which you can find and read at your leisure in any standard work on diseases of the eye. Such works are numerous, and I dare say each one of you possesses one or more of them in your library, and that many of you are quite as conversant with the subject matter of them as I am. They all relate nearly the same story, excepting little differences here and there of minor importance. From them you have learned that ulceration of the cornea is the result of disturbed nutrition, and that this may be produced: 1st, by a vitiated or improper state of the blood; 2d, by an irregular and deficient supply of the blood; 3d, by a disturbance or a loss of the nervous influence; and 4th, by an unnatural state of the cornea itself.

It is especially rare, however, in practice to meet with cases that illustrate exclusively any of these pathological conditions except the one resulting from a disturbance or a loss of the nervous influence. I have seen very meagre mention of this being assigned singly as a cause of the ulceration of the cornea in any of the numerous and well-written text-books on the subject of ophthalmic surgery. This may explain the fact that the nervous origin of corneal ulcers has been too frequently overlooked even by excellent observers. But I think when the attention has been once directed to it, the symptoms are very plain, peculiar and characteristic, and will scarcely ever elude the attention of even a superficial observer. The extent, occupying as I have seen it in a very few instances, the anterior surface of the globe and the conjunctival sac, the dusky, purplish appearance of the conjunctiva, the marked absence of the secretion, the dry and leathery appearance of the entire surface of the cornea, the absence of photophobia even when exposed to very bright light, the

[1] Read before the Chippewa Valley Medical and Surgical Association at Eau Claire, Wis., May 11, 1886.

almost or quite complete anæsthesia of the cornea and the conjunctiva, and the entire absence of pain, form a picture the impression of which will scarcely be forgotten. It is useless for me to speculate as to the exact nature and locality of the pathological conditions that give rise to this lesion of the eye. It is a fact pretty conclusively demonstrated by experimental physiology, that the removal of the superior cervical sympathetic ganglion produces disturbance of the nutrition of the eye, and especially of the cornea. But whether it be through the direct influence of the sympathetic or whether it be through the loss of the action of this nerve on the fifth or the trifacial, has not been very clearly determined. It is, however, a well established fact that a division of the fifth does produce a very decided disturbance of the nutrition of the cornea, and the entire conjunctival sac.

In the case that I will relate I was unable to trace the origin of the nervous disturbance beyond the fifth, or tri-facial, since the case had been under progress for several days before my advice was sought. I have selected this case out of the few that I have had an opportunity of seeing, because it is typical and furnishes a more pure and uncomplicated illustration of nervous disturbance in the production of corneal ulceration than any other that ever came under my observation. The patient was a blacksmith by occupation, about 50 years of age. For a period of nearly thirty years he had been a very industrious laborer, and all this time he had scarcely known a day of illness prior to the affliction for which he sought my advice. He related that about a fortnight since, after a disturbed and restless sleep, he awakened in the morning with a pain in the left temple and side of the face, and slight dizziness. He went to his shop, however, and continued to work uninterruptedly through the entire day. The pain during the day became more tolerant at times, and he thought it to be only an ordinary attack of neuralgia, and that it would pass away under the influence of some domestic remedies which he would employ in the evening. The next morning there was, in addition to the pain, a prickling sensation, and the dizziness was somewhat increased. He felt some alarm about his condition, and sought the advice of his family physician, who gave the case a pretty thorough examination, and concluded that if it was neuralgia it was a very extraordinary case.

In short, his symptoms continued to grow more alarming, and he soon began to observe some circumcorneal injection of the left eye, which was at first somewhat sensitive to the light. This, with some slight increase, remained several days. To follow his description, as the dizziness increased the pain diminished, and the face drew to the right side. The facial paralysis was complete both as to motion and sensation. The vision of the left eye began to be hazy. The haziness increased rapidly, and when he consulted me he was scarcely able, with this eye, to determine the location of the window in my office. The ocular tunic was quite vascular, with a considerable amount of chemosis about the cornea. The entire conjunctiva presented a dry, glossy, purplish appearance. The cornea was dry, leathery and nearly opaque. In the centre there was a superficial ulcer with sharp, well defined edges. There was entire absence of pain, and the anæsthesia was so complete that the finger could be rubbed all around in the conjunctival sac and over the cornea without producing any discomfort. There was complete anæsthesia of all the parts supplied by the cutaneous nerves of the ophthalmic division of the fifth. Of course, the affection of the eye for which he especially consulted me was quite beyond the resources of any known means of relief. The destruction of vision in this eye became complete in a few days. The paralysis, shortly after I saw the case, suddenly extended to other parts of the body, and soon terminated fatally. An autopsy could not be obtained, and therefore the nature and origin of the brain lesion was left somewhat a subject of speculation.

In connection with ulcers of the cornea I wish to call the attention of the profession to the use of a certain astringent, not for the purpose of extolling it but rather for the purpose of condemning it, and my experience with it teaches me that I cannot condemn it in terms too severely. It is an astringent too that is recommended promiscuously in every form of conjunctivitis by all the modern works on diseases of the eye that I have been able to consult. It is an old remedy and perhaps one of the most reliable astringents in the materia medica. There is, perhaps, not a writer on this subject who has not spoken of it in terms of praise in conjunctivitis. I have treated a great many cases of conjunctivitis, and it was a very common practice for me to combine this astringent with other astringents. I regarded it as one of the safest and most reliable remedies that could be applied to an inflamed conjunctiva. Had it not been for a custom that I long since adopted in my ophthalmic practice of keeping a pretty complete record of my cases, I should, perhaps, have been employing the same old astringent to-day with all the evil results it is capable of producing. In all cases of conjunctivitis it is a very common custom to inspect the cornea closely before prescribing an astringent. I was many times chagrined that within twenty-four or forty-eight hours after I had prescribed an astringent for a simple conjunctivitis, to find in the periphery of the cornea one or more minute ulcers. It did not seem possible that they could have escaped my detection the day before. There seemed to be something wrong, and I determined if possible to learn the cause. I consulted my records very carefully and discovered that no case of ulceration had occurred in any of my cases of conjunctivitis where a simple astringent had been employed without alum, and that in every case where ulceration followed, alum had been prescribed singly or in combination with some of the other sulphates. This seemed evidently the key-note to the ulceration. I at once procured the fresh cornea of a hog's eye, cut it into small pieces and immersed them in a solution of alum of various strengths, and in every instance, at the end of twenty-four or thirty-six hours, I found all the cementum of the cornea dissolved and nothing but the stroma and the anterior and the posterior

elastic lamina remaining. I have verified this a number of times.

I have long since discarded alum, and ulcerations of the cornea in conjunctivitis have ceased to annoy me. In those cases in which alum had been used without producing ulceration, the anterior elastic lamina had been intact, but if there had existed a minute break in the covering and the alum had had a free access to the proper substance of the cornea, it had invariably formed an ulcer. Every valuable text-book on ophthalmology that I have been able to consult, recommends alum in high terms not only in simple conjunctivitis, but also in purulent and gonorrhœal. This statement in regard to the action of alum may seem a little incredible to some, but I only ask them to subject it to a fair, impartial test, and I have no hesitancy in saying that their infidelity will be cured, and alum will no longer find a place in their ophthalmic therapeutics.[1]

Herpes of the cornea frequently degenerates into chronic ulcers. When this occurs in a child it is a very common practice to charge it to the account of scrofula or a scrofulous taint, and if the child should be a healthy subject, it is said to be due to a latent scrofulous predisposition. The fact is, herpes that so frequently degenerates into small corneal ulcers is, as a rule, "not the localization of a blood disease," but rather the scrofulous appearances are the result of the herpes and ulceration. The enlargement of the concatenated glands of the neck are, in a majority of cases, nothing more nor less than a reflection from the ulceration, just as a "felon on the finger may produce an enlargement of the axillary glands." These little cavities or ulcers formed from the degeneration of the herpes, under a proper treatment, fill up with transparent cornea, and every trace of them may be lost. Sometimes the newly formed corneal tissue is cloudy at first, but subsequently clears and becomes entirely transparent. Occasionally, however, I have seen them fill with a kind of chalky material that becomes a fixture and remains through life.

Pathology teaches that in the healing of corneal ulcers new cells must be formed to take the place of those that have been detached and destroyed. It has been somewhat conclusively shown that the new cells are derived from a finely granular blastema which changes into protoplasm, and that in this protoplasm new cells arise by a process of free cell-formation. "Again, Alexander Rollett maintains that this corneal protoplasm is contractile. He arrives at this conclusion by experiments upon the fresh cornea of a frog. He submitted it to the action of electrical currents. Before the passage of the current the corneal tissue seems to be homogeneous, or with only a few radiated corneal corpuscles scattered through it. Immediately after the excitation, elongated and sinuous, fusiform, elliptical, and round, clear figures make their appearance. These are nothing but the optical expression of longitudinal, oblique or transverse sections of the communicating system of lacunæ traversing the cornea, the dilated nodal points of which are occupied by the nucleated, central portion of the radiating and contractile corneal corpuscles. The contours of this canalicular system are rendered visible by the electrical current, because it occasions the retraction of the corneal corpuscles from the walls of the cavities in which they are contained. In all such cases the nucleated masses of protoplasm may be seen surrounded by a transparent area, and with small spikes projecting from its surface, which are the retracted arms. The appearances described vanish if the shock is not repeated. They can, however, be made to appear again if the current is retransmitted."

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[1] Those who may have observed this peculiar action of alum on the cornea will confer a favor on the author by communicating the fact, either by letter or otherwise.

MEDICAL PROGRESS.

ELECTROLYSIS IN THE TREATMENT OF STRICTURE.—At the meeting of the Royal Medical and Chirurgical Society on May 25, DR. W. E. STEAVENSON and MR. W. BRUCE CLARKE read a paper on this subject. The more extensive use made of electrolysis in surgery and gynæcology abroad, and especially its successful employment in the treatment of stricture of the urethra, had induced the authors to undertake a series of observations to test the accuracy of the reports which had reached this country. Their results bore out in every particular the results of success they had received from America. Electricity, on account of its power of splitting up compounds into their chemical elements, could be used as a substitute for ordinary caustics to the human body. It could be used with especial advantage to parts difficult of access, such as the male urethra and the uterine cervical canal, and it could also be applied to these and other regions where the application of other caustics was attended with a certain amount of danger. Its effects could be limited to the points touched by the electrode. The caustic effect could be arrested, or not commenced until the applicator, in the form of the electrode, was *in situ*; and the duration and extent of the caustic action were entirely under the control of the will of the operator. The treatment of stricture of the urethra by this method was the most simple, and perhaps the most striking in its results, and had, therefore, been selected as the first on which to collect and report observations. In this paper, the details were given of six cases of stricture of the urethra treated by electrolysis, the *modus operandi* was explained, the steps of the operation were given, and the advantages of this method of procedure were summed up. There was usually no bleeding. If hæmorrhage did occur, it was accidental, and usually showed that too strong a current had been used; no anæsthetic was required. If pain or discomfort were produced, it was trifling. The patient could, in the case of slight strictures, pursue his ordinary occupation during the period of treatment. No antiseptics were required, as the process itself was aseptic. In the majority of cases, there was no contraction or return of the stricture. Eschars pro-

duced by caustic alkalies were said to heal with less contraction than wounds produced in any other, and electrolysis with the negative pole of a battery was a means of applying the same destructive action as was caused by the caustic alkalies to parts difficult of access in a way which was impossible by any other method. Probably, other chemical decompositions and combinations took place at the negative pole besides those characteristic of the caustic alkalies, but they had not, up to the present time, been thoroughly made out.

Mr. Berkeley Hill was much interested by the paper, but was sorry to say that he could not find the conclusions of the authors quite satisfactory. The cases had been related, after the manner of the American writers on this subject, without enough detail as to the size, nature, and position of the stricture. One case had suffered from stricture, more or less for twenty years; he had often been relieved for a time by ordinary dilatation, and then had slowly relapsed. The relief given by Dr. Steavenson and Mr. Bruce Clarke dated only from about eight months ago, and he expected that his discomforts would return; at any rate, at present, there was no proof of a cure. It was universally admitted that, by patient pressure, a larger catheter could be passed; but he thought the authors had hardly made due allowance for this in their account of the increased size of the electrical bougies passed, which might perhaps be due to patience as much as electricity. He had himself carefully considered the American cases that Dr. Newman had published, and had followed his plan in treating one case of his own. There was stricture two inches and a half from the meatus, probably behind an old urethral abscess; he could see clearly with an endoscope, which he was sorry the authors had not used. He passed a needle, which formed one pole of the electric circuit, into the scar-tissue, and could see, with the endoscope, that nothing more happened than the occasional liberation of a bubble. The patient felt no pain at all, except on making and breaking the current; he measured the currents strength by the patient's feelings. After fifteen minutes application of the current, he found the stricture enlarged from 19 to 20 of the French scale. After further treatment, in the same manner, it gradually contracted from 20 to 16; and as cure semed very unlikely by such a process, he resorted to other methods, and found no difficulty in widening it.

Mr. F. Swinford Edwards had treated a case, with the help of Dr. Steavenson. He had first seen the patient two and a half years ago, when he found three strictures, two penile and one subpubic, of size No. 12 (French); these he gradually dilated to No. 25. For two years, the patient was lost sight of, and then returned with three strictures, distant ½ inch, 4 inches, and 5½ inches from the meatus, admitting only No 4 (French). Dr. Steavenson applied a No. 3 electrode and current of from 5 to 8 milliampères; this did not pass; but the flow of urine was improved, and a few days later, a bougie, No. 10, could be passed; and, after a few weeks under electrical treatment, a bougie No. 26 steel or No. 28 pewter, could be admitted. Whether it was to be called strictly a cure or not, it was certainly satisfactory in enabling the patient to get about and to do his work; and his case was a bad one, which would otherwise have needed treatment with an Otis's dilating urethrotome. He hoped the treatment would be further investigated in hospital cases.

Mr. G. Buckston Browne thoroughly agreed that it would be a very good thing if the practised hands of the seniors in the profession could be brought to try this method; for there was hardly any point in which more experience was needed than in estimating and accurately diagnosing stricture. He was inclined himself to attribute the results embodied in the paper to the dilatation, not to the electricity; else why was it found advisable to use gradually larger and larger electrodes? Even pressure, without passage of a catheter, often did much to facilitate the passage of urine. He had considered Dr. Newman's cases with some care, and could not help calling attention to one remarkable point in them, that they were, every one of them, successful.

Dr. Steavenson was not familiar with the endoscope which Mr. Berkeley had shown. In Mr. Hill's cases, he understood there had been puncture, and he certainly was not surprised, that contraction had followed. They had used bougies, of gradually increasing size, to keep in contact with the walls of the urethra.

Mr. Bruce Clarke said he had been quite prepared to meet the two chief objections that had been urged; that the cases were not really of organic stricture; and that they were not really cured. As to cure, he was bound to admit that the operations had been performed last August, and not treated since; they were under observation, and had not relapsed; only time could prove their ultimate cure. As to the point that they were spasmodic strictures, he could not admit it; in one of the cases, there had been extravasation of urine; at first, nothing could be introduced, but after electrical treatment, he could pass No. 11 (English), and that was a success as great as any after a cutting operation.

The British Medical Journal, also says editorially: Interest in the subject was first aroused by one of the daring attempts of American surgery, made by Dr. R. Newman, of New York, who published, about eighteen months ago, *Tabular Statistics of 100 Cases of Urethral Stricture treated by Electrolysis, without Relapse.* It would have appealed, perhaps, even more strongly to those who believe in the fallibility of human nature, if there had been one or two relapses. The fashion of operating admits of many small variations; but the essential points are, that one pole of the battery shall be of metal, and in contact with the surface of the stricture, and the other widely spread out by means of a pad over a considerable surface of the body, the back or elsewhere; and that between these poles a current of considerable strength should be passed. It is found most successful and least uncomfortable that the negative pole should be in contact with the urethra; the positive, with the body. A current may be passed which is strong enough to act upon the stricture without giving any discomfort, except, perhaps, at the moments of mak-

ing and breaking. What the exact action of the current upon the cicatricial tissue may be, we are hardly in a position to say, though the actual watching of the process by means of an endoscope, as practised once by Mr. Berkeley Hill, may throw more light upon it. It is covered, at present, by the word "electrolysis," of which, when applied to fibrous tissues, we must admit that the limits are somewhat indistinct. At any rate, it is alleged that not only does the resistance of the stricture give way, but that more or less of the tissue which forms it is turned into a slimy mass of broken-down epithelium, and so disappears, without leaving a contracting cicatrix. The *à priori* impression is certainly strong that, where tissue has disappeared, there must be a cicatrix; and that, if there is a cicatrix, it must contract sooner or later. Those who have practised electrolysis will gain a much more attentive hearing and a more zealous following when they can show a longer maintenance of good results than the eight months which have elapsed since Mr. Bruce Clark's operations. When the malady is chronic, it naturally needs a long time to judge of the cure; but, at the same time, the habits of scars, due to different causes, are known to vary greatly in contraction, and it is possible enough that there may be less contraction after electrical action than any other, even than those from caustic alkalies. And, further, we are not yet experienced enough to assert how completely similar or dissimilar to an ordinary cicatrix this process of electrolysis may be. That it deserves trial from the older and most skilled hands, there would seem to us little doubt; and we can imagine it most convenient that the electrical necessaries should be managed by the younger students of that somewhat difficult class of instruments.

BERI-BERI.—An industrous investigation of some of the circulatory conditions that obtain in the Japanese "kakké" or beri-beri has been undertaken by DR. WALLACE TAYLOR, with the production of some valuable results. In none of the cases was there organic disease of the heart or arteries, and no functional derangement other than that attributed to the influence of the "kakké." Sphygmographic tracings revealed a very sudden and high upstroke, with precipitous descent from the apex of the percussion wave and marked dicrotism. The first deviation of the circulatory system in kakké from the normal is one of cardiac excitement. There is also diminished tension, due to loss of arterial tone. The chief causes of danger in the acute disease are found in the impaired condition of the heart and enfeebled circulation. The extent, however, to which the heart and vaso-motor system are affected in the disease is liable to relatively large changes. Dr. Taylor considers that all the conditions of the heart and arteries can be explained by the action of the *materies morbi* of kakké upon certain portions of the cerebro-spinal nerves and sympathetic system. Age exercises considerable influence over the occurrence of the disease, which is most common between 16 and 28 years of age. One attack of kakké appears to render the patient more liable to subsequent affection. Women are much less likely to be affected than men. Though the female sex in general enjoy a marked immunity, yet in the puerperal state they are very liable to take the disease. It is but seldom that traces of albumen are found in the urine. Partial paralysis of the muscular walls of the bladder is occasionally to be met with. No mention is made of the multiple neuritis, of which so much has lately been written in England. —*Lancet*, May 8, 1886.

PARALYSIS OF LARYNGEAL MUSCLES.—The generalization so strongly upheld by DR. FELIX SEMON, that the abductor muscles of the larynx are more prone to paralysis than the adductors does not carry us a great way, but it is very useful as a step to further knowledge, A general cause acting on the recurrent laryngeal nerves appears always to paralyze or weaken the abductor muscles before or more than the adductor. M. Charazac records a case in the *Revue Mensuelle de Laryngologie* of cystic goitre associated with unilateral paralysis of the adductor muscles of the larynx consecutive to compression of the right recurrent laryngeal nerve. During expiration the larynx appeared to be perfectly healthy; the vocal cords left the middle line in a natural manner, so that the glottis presented its normal triangular outline. But during phonation the left vocal cord closed up to the mesial line in good form, but the right did not approach its fellow. We think M. Charazac lays too much stress on his single observation, supposing that to have been a true one, and not to be explained by other causes than the one to which it is attributed. So far the generalization to which we above referred seems to have been confirmed and strengthened by numerous and carefully observed cases. — *Lancet*, May 22, 1886.

FALSE JOINT FROM A FRACTURE IN INFANCY.—M. BERGER recently showed a very interesting specimen at a meeting of the Société de Chirurgie of Paris. It consisted of the lower limb of a man 57 years of age, who had received a fracture of both bones of the leg when only nine months old. This was followed by the formation of a false joint which prevented the man from walking. The upper fragments of the tibia, and fibula were rounded off, and fitted into cup-like depressions on the lower fragments. The adjacent surfaces were eburnated and coated with dense granulation tissue (not cartilage), and were held together by a capsular ligament continuous with the periosteum and lined by a true synovial membrane. The knee and ankle were not ankylosed, as is often the case in such conditions. The whole limb was undeveloped, as the following measurements on the two sides show: Length of foot 18 centim. as compared with 24 centim.; length of patella 5.0 centim., instead of 6.0 centim.; length of femur 36 centim., as against 39 centim. The muscles were not degenerated; the nerves to the naked eye were healthy; but the skin over the toes was the seat of æsthesia and local asphyxia. The question raised by M. Berger was, whether these developmental changes were the results of the "false joint," or whether they and it were both due to one obscure trophic affection.—*Lancet*, May 8, 1886.

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THE EDITOR OF THIS JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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THE BLOOD-PLAQUES AND COAGULATION.

In the last number of THE JOURNAL we attempted to give some idea of the blood-plaque, and showed that while there is very strong evidence that it is an independent element of the blood, there are also strong reasons for believing that it is what Hayem terms an hæmotoblast, or young red corpuscle. But which of the theories be held matters but little in considering the relation of the plaque to the process of coagulation. This however, is best prefaced by some account of the histology of the formation of fibrin, and its relation to the histological elements of the blood, including the blood plaques.

In the study of the formation of fibrin under the microscope it has long been noticed that the fibrin filaments spread out as rather distinct rays from the minute aggregations known as Schultze's granular masses. This was noticed by Schultze, and by Ranvier in 1873, who regarded them as centres of coagulation; and also by Riess, Hayem, Leube, Bizzozero and others. Ranvier, Davidson, Bizzozero and Halla think that there is a distinct and direct connection between the fibrin-threads and the granular masses, but Schultze thinks that the threads do not proceed from the masses, but pass over or through them. Heyl, having observed that in Bizzozero's fluid the number of granular masses increases as the leucocytes decrease, and that both the granular masses and leucocytes stain with methyl violet, has drawn the unwarranted conclusion that, on the assumption that the leucocytes break down during coagulation, the granular masses are derived from the leucocytes. This view has received the support of Hlava and Riess, but is denied emphatically by Osler and Laker; and Löwit, after a careful study of the coagulation of lymph under the microscope, has found nothing that can be regarded as leucocyte detritus. The generally accepted view as to the granular masses, then, is that they are formed by the plaques.

In the very careful study made by Kemp he found that the formation of the granular masses by the plaques may be seen in the following manner: The finger is pricked and a good-sized drop of blood is squeezed out. This is taken immediately upon a cover-slip, and then as quickly as possible most of it is washed off by a jet of a 75 per cent. NaCl solution from a wash-bottle. The cover-slip is now placed on a slide, and transferred to the microscopic stage as quickly as possible. The plaques have the property of sticking to the slip, while the other elements are easily washed away by the jet, so that upon examination the whole field will be seen to be filled with plaques, some of them isolated, but most of them grouped in masses consisting of from two or three to a dozen or more. They are now not pale and homogeneous, with a symmetrical outline, but appear glistening and granular, and their contour, instead of being regularly oval or circular, has become jagged. These changes are the more marked the longer the time which has elapsed before the preparation is observed; and they may be seen to take place step by step while the observer watches the preparation. The plaques continue to undergo changes in form, until finally when they are grouped together only a granular mass is found, and the individual plaques can no longer be distinguished clearly. "*Pari passu* with these changes processes are seen which run out from the granular masses; and when coagulation sets in these processes are nearly always found to be continuous with threads of fibrin. The threads of fibrin are sometimes deposited as long needle-shaped crystalloids which are often seen lying free in the field, and not connected with the granular masses; but the greater number are formed most thickly around these masses, from which they often radiate as centres."

Kemp has found that when the blood-plasma is very much diluted but little fibrin is formed. "Under these circumstances the field is comparatively clear, and it can then be seen that no fibrin proceeds from the leucocytes, but all comes from the plaques, or is deposited freely in the field." Hayem and Schimmelbusch have pointed out that fibrin may be formed elsewhere than around the plaques, and this view has been confirmed by Osler; though Bizzozero has stated that fibrin is deposited around the plaques and nowhere else. Osler says in his third lecture

that it may be noticed that fibrin appears quite independently of the plaques, particularly if healthy blood be examined in which plaques are not very numerous. When there is an abundant formation of fibrin the network of threads is seen all over the field, and every spot is full of fibrin; but when the fibrin is not abundant it will almost always be seen that the fibrin is thickest in the vicinity of the masses of plaques. "Even in preparations where the fibrin is sparingly formed, the threads are deposited elsewhere than around the granular masses, and occasionally, though rarely, I have found granular masses around which the fibrin did not appear to lie more thickly than in the clear field. The fact that nearly always the fibrin is deposited most thickly around the granular masses, even radiating from them as centres, while interesting and significant, is not conclusive proof that the plaques are connected with coagulation; for the same adhesive property of the plaques which makes them adhere to each other, may also cause the threads of fibrin to stick fast as they separate out from the medium around them. This seems all the more probable when we consider that the fibrin as well as the plaques is sticky and adheres to the glass" (Kemp). The fact that in preparations in which the clot is scanty the fibrin is deposited more thickly in the vicinity of the masses of plaques may possibly be due to the plaques giving up something which causes or hastens coagulation, and that in dilute solutions this substance is more abundant in the vicinity of the granular masses than elsewhere.

Laker has taken the ground that the fibrin-threads are folds of a membrane which he calls the primary fibrin membrane; a view almost identical with that taken by Virchow in 1856. Kemp concludes that what Laker describes as a membrane is a layer of the homogeneous substance described by Virchow, Rindfleisch and Hermann, which is essentially of the same composition as fibrin, and from which the fibrin-threads are formed by a process very closely resembling crystallization, if not identical with it. Hermann thinks that the threads are formed by a process which closely resembles crystallization, and Schimmelbusch insists most positively that the formation of fibrin-threads is a true crystallization process, nor does he believe that there is a previous stage, either homogeneous or granular. Hayem, though not believing in the fibrinous character of the plaques, compares the formation of fibrin to a sort of crystallization starting from small crystals already formed. Kemp says: The definite form which the fibrin-threads take, especially the typical needle-shaped form of the threads when deposited isolated in scanty clot, speaks strongly in favor of a crystallization; while the subsequent toughening and contraction of the threads show a clear resemblance to the coagulation of certain proteids, notably myosin. In fact, it appears that we have in blood an interesting process which may be regarded as intermediate, in a certain sense, between a true crystallization on the one hand, and the coagulation of certain proteids, as myosin, etc., on the other. Hence Kemp thinks it evident that there is no histological connection between the plaques and fibrin, so that if the former are involved at all in coagulation the connection must be a chemical one; in other words, the plaques must give up something to the plasma at the same time that they break down. But the strongest evidence of a connection between the plaques and coagulation is derived from facts pointed out by Hayem, Bizzozero, Lavdovsky, Halla and Ferraro: that fibrin is formed *pari passu* with the breaking down of the plaques; and that reagents or conditions which retard the breaking down of the plaques, retard to *precisely the same extent* the formation of fibrin; and reagents which *preserve the plaques prevent the formation of fibrin altogether.* Kemp's observations, so far as completed, confirm these facts in every respect.

The question now arises, what part do the plaques play in the process of coagulation? Hayem and Bizzozero think that their part is to furnish something essential to coagulation, and they agree that *ferment* is in all probability the agent in question. Now, in some experiments Kemp noticed that blood flowing through a dirty tube coagulated more quickly than in flowing through a clean glass tube; and he thinks that this would seem to show that in passing through the dirty tube the blood took up something which brought about coagulation before the plaques broke down, but which is also formed later by the plaques when they break down. And from what we know of coagulation at the present time, he says, it seems probable that the agent in fibrin-formation that would be more likely to act in this manner is the *ferment.* Osler has examined the relation of the blood-plaque to coagulation experimentally, and his results are very interesting. If an ordinary ligature, partly teased out, be passed through the femoral vein of a dog and allowed to remain for five or six minutes, or less, the threads become coated with the plaques; and the same coating of plaques may be obtained by whipping freshly drawn blood with a bunch of threads, as in Bizzozero's experiment. The threads are then carefully washed in a saline solution, by which the red corpuscles are removed. If the threads be then dipped into a coagulable solution clotting occurs.

But Rauschenbach criticizes this experiment by saying that the threads may have absorbed ferment which was not washed away; and that the coagulable fluid used by Bizzozero is only a test for free ferment. Kemp thinks that the first objection is valid, but Osler replies to it by saying that the chief elements in the clot are plaques; and the greater the number of the plaques the denser the coagulum. To the second Kemp replies that by granting it we exclude the leucocytes adhering to the threads from a share in the formation of the ferment, if any is formed by what does adhere to the threads. It then remains to explain why the time of coagulation should depend upon the number of plaques, and the experiment remains of value as offering strong support to the theory that the ferment may be derived from the plaques.

But there is further and more conclusive evidence of the participation of the plaques in the process of coagulation, as afforded by the experimental production of thrombi. So far as completed Kemp's work in this direction tends to confirm the results of Osler, Bizzozero, Hayem, Ferraro and Lubnitzky, who find that the white thrombus is not composed of leucocytes but of plaques; that is due to an agglomeration of plaques around a lesion in the vascular wall, or a foreigr body introduced into the vessel. The plaques are the elements which first settle on the edge of the wounded vessel, and form the basis of the clot, as shown by Eberth, though he still holds to the idea that the leucocytes play an important part in the formation of white thrombi. Osler contends that they are composed chiefly of plaques, and that their further development results from the disintegration of the plaques; that a granular or stroma-fibrin and a fibrillar or plasma-fibrin must be recognized, the former being identical with Weigert's coagulation-necrosis.

The conclusions drawn by Kemp regarding the relation of plaques to coagulation are: 1. When the blood is drawn the plaques break down almost immediately. This is not true of any other element of the blood. 2. The breaking down of the plaques is intimately connected, in times at least, with the clotting of the blood. 3. The connection between the breaking down of the plaques and the coagulation of the blood is not histological, but chemical—*i.e.*, the plaques appear to give up a soluble substance which is active in coagulation. 4. The active agent in question is most probably *fibrin-ferment.* 5. Fibrin is deposited histologically independent of any of the cellular elements of the blood. 6. When the clot is very scant fibrin is deposited as long, needle-shaped, crystal-like bodies.

"METAPHYSICIANS," "FAITH-HEALERS," AND THE ILLINOIS PRACTICE ACT.

The Illinois Practice Act is a law, coming under the police power of the State, and is maintained on the theory that the law is enacted for the preservation of the public health from the experiments of quacks and unqualified persons; and, says Attorney General McCartney, of Illinois, it must be construed as an absolute prohibition against the practice of medicine in this State, except under the conditions specified in the Act. These conditions are that the person practising, if he practises under a diploma from a medical college—shall have a certificate from the Board of Health of the State that his diploma is genuine; or if he is not practising under a diploma, that he has been examined and found duly qualified by the State Board of Health; or that he is a commissioned surgeon of the United States Army or Navy; or that he had been practising medicine ten years in the State of Illinois prior to the first day of July, 1877; or that he was a student prescribing under the supervision of preceptors; or was performing gratuitous services in a case of emergency. These are the conditions, and these alone, under which a person may "profess publicly to be a physician and to prescribe for the sick, or to append to his name the letters of M.D."

Now what is it to practise medicine? Is it necessary that the person practising shall administer drugs, or that he shall perform a surgical operation? Would any court in the land limit the term to this definition? Does not "practising medicine" mean that the person practising professes to cure disease, or to assist in the cure of disease or bodily ailments? We may easily suppose the case (for such a one is on record) in which a distinguished consulting physician continues in practice for several years without prescribing a dose of medicine once in two years, or longer. This man is certainly practising medicine. And inasmuch as the "metaphysicians," "faith-healers" profess to cure disease and bodily ailments, and in some cases append the letters M.D. to their names, though signifying on their signs that they are "metaphysicians" or "faith-healers," are they not indictable under the Illinois Practice Act for practising medicine without licenses? At least one of these people has the letters M.D. appended to his name on his sign in Chicago. From long custom the public understands that those letters mean Doctor of Medicine; though the person misappropriating them may say that they mean "Money Down," "Milk Diet," or "Medicine Disgusts."

The Secretary of the Illinois State Board of Health, in response to a question as to the legality of these

people practising, says that he doubts if they could be prosecuted, but that he thinks that they could not collect fees should the person receiving the "treatment" decline to pay. It seems, however, that the only ground upon which a court could refuse to enter judgment for the plaintiff in such a case would be that the plaintiff was practising medicine without a license, and therefore could not collect fees by law. It is barely possible that a court might refuse to give judgment on account of "want of consideration;" but this is only barely possible, and the plaintiff could almost certainly carry the case up on error. But should the decision be as first suggested, the question at once arises: If payment may be refused on account of illegal practice, why cannot the plaintiff be indicted for this illegal practice?

As evidence of the correctness of our definition of "practising medicine," we may cite the case of Bibber vs. Simpson (59 Me., 181), in which suit was instituted for $51 for services rendered the defendants intestate, at his special request, by the plaintiff as a clairvoyant. It appeared from the plaintiff's testimony that she professed to be a clairvoyant; that when asked to examine the patient she saw the disease and felt as the patient did; that sittings or séances were of different durations, from one-quarter to one-half of an hour each; that she did not pretend to understand anatomy or medicine; that she was requested by the intestate to visit him and render him professional services, and did so as by the account; that she helped him, but he died from taking cold; acquainted him with the prices, and he agreed to pay them, but never did so. Appleton, C. J., in deciding the case, said: "The services rendered are medical in their character. True, the plaintiff does not call herself a physician, but she visits her sick patients, examines their condition, determines the nature of the disease and prescribes the remedies deemed by her most appropriate. Whether the plaintiff calls herself a medical clairvoyant, or a clairvoyant physician, or a clear-seeing physician, matters little; assuredly, such services as the plaintiff claims to have rendered, purport to be, and are to be deemed, medical."

Not only must the services rendered by these parties be deemed medical, but it must be held that the parties rendering these services publicly profess to be physicians; since a physician is one who claims to heal the sick. And, says Attorney General McCartney, if it should be charged in an indictment or complaint that A. B., on etc., etc., practised medicine without then and there possessing the qualifications prescribed in the certain act of the General Assembly of the State of Illinois, entitled "An Act to Regulate the Practice of Medicine in the State of Illinois," approved May 29, 1877, and without then and there having complied with the provisions of that act, and under this charge the prosecution should prove beyond a reasonable doubt that the defendent A. B. did, within eighteen months prior to the finding of the indictment, prescribe for the sick while publicly professing to be a physician, or that he did append to his name the letters "M.D.," a conviction must necessarily follow, unless he should produce his certificate of the State Board of Health, his commission as surgeon in the United States Army or Navy, or prove himself to have so acted under one of the other conditions prescribed in the statute.

RABIES.—A farmer from Roumania, who had been bitten by a mad dog, was placed under Pasteur's treatment in Paris. Notwithstanding, the disease developed in due time and proved fatal. This makes the *eighth* death from hydrophobia after having been subject to a pretty fair test by inoculations by Pasteur himself. The Roumanian was reported to have been bitten only thirteen days before coming under treatment by Pasteur.

CHOLERA.—Late advices represent cholera as prevailing with considerable activity in Venice, and two deaths from the same disease had taken place at Florence, Italy.

SOCIETY PROCEEDINGS.

AMERICAN LARYNGOLOGICAL ASSOCIATION.

Eighth Annual Meeting, held in the Hall of the College of Physicians, of Philadelphia, May 27, 28 and 29, 1886.

(*Continued from page 667.*)

DR. F. I. KNIGHT, of Boston, read a paper on

LARYNGEAL VERTIGO.

A number of cases of what is called laryngeal vertigo have been reported by various authors. The distinctive feature of these cases is that attacks of coughing are followed by giddiness and momentary loss of consciousness. This latter symptom was present in all but two cases. While it is probable that attacks of dizziness after coughing are not rare, yet but little attention has been paid to the subject. The writer had been able to find fourteen published cases. To these he adds two coming under his own observation. The first case was that of a man aged 42 years, who had a chronic bronchitis of one year's duration. He had had rheumatism when 15 years of age, and on one occasion had suffered with insom-

nia. No morbid condition of the heart was detected. The chest was filled with sonorous and sibilant râles. Under the use of iodide of potassium there was gradual improvement. After a day of worry and fatigue, he had a cough followed by loss of consciousness. There was no evidence of spasm of the glottis and no convulsive movements. He never had another attack of loss of consciousness.

The second case was that of a lady 47 years of age, who had previously been the subject of attacks of dizziness, the result of indigestion. She had suffered with winter cough for a number of years. During one attack of coughing she lost consciousness. During this time there was congestion of the face.

The following points were obtained from a study of the reported cases: All the cases with the exception of one have been in males. The average at which the attack has occurred has been 47½ years. The cough which induced the attack was slight in six cases, spasmodic in two, and severe in three. Momentary loss of consciousness occurred in fourteen cases. One case fell, but declared that he had not lost consciousness. Dizziness is mentioned in eleven cases. In four cases there seems to have been decided evidence of laryngeal spasm. In one this was doubtful. In four cases there was marked congestion of the head and face. In two the patients were pale. Convulsive movements of the limbs occurred in three cases. In one the movements affected the face and head, and in another the face. In these cases there was no biting of the tongue, no frothing of the mouth, and no involuntary micturition. The speaker objected to the term laryngeal vertigo. There probably is no real vertigo in these cases in the sense of the vertigo seen in aural vertigo. There is simply giddiness or lightness of the head. These cases can not be considered to be instances of petit mal.

The cause of the attacks is disturbance of the cerebral circulation, perhaps due to the compression of the large blood vessels of the chest and perhaps the heart. Even without this, the effect of rapid breathing is well known. This causes marked cerebral disturbance.

Dr. D. Bryson Delavan, of New York, read a paper on

BUCCAL TUBERCULOSIS.

He had seen six cases of this affection. In the reports of twenty-four cases of tuberculosis of the tongue, in all but one the subjects were males. In twelve cases the lesion was anterior, in seven upon the side, in one at the base of the tongue, and in four the situation was not stated. The disease was primary in nine cases and secondary in seven cases. In the remaining cases it was not stated whether the disease was primary or secondary. The longest duration of a case of primary tuberculosis was two and one-half years, the shortest ten weeks. The age of the oldest person affected was 70 years, and of the youngest, not under 12.

The first case coming under the speaker's observation was H., aged 35, a laborer. His family history and previous history was good. The man was remarkably robust. Six weeks before coming under observation, he experienced pain at the base of the tongue. This pain became excessive, and at the end of two weeks he was obliged to give up work. On examination a fissured and ulcerated surface was found at the base of the tongue on the right side. This lesion was indurated. In the centre was a large ulcer with large flabby granulations. On the right side, externally under the angle of the jaw was a hard tumor about the size of a chestnut. There was slight fetor of the breath. The diagnosis was uncertain, the pain was so severe as to prevent sleep. An application of a 4 per cent. solution of cocaine checked the pain for five hours. The patient's condition became so serious that he was removed to the New York Hospital, where under the idea that the disease was epitheliomatous, the complete removal of the tongue was made. The patient recovered rapidly from the operation and there was marked improvement in the general condition. It has been impossible to find any evidence of disease of the lungs or other internal organs. On microscopic examination all the parts present abundant tuberculous material. A point of interest in this case, is that this is the first case on record in which such an operation has been followed by an apparent cure. Of course sufficient time has not elapsed, to speak positively as to the ultimate result. The operation was done in the latter part of December, 1885.

In a second case of tubercle of the tongue, the patient, P. O., age 27, gave no history of hereditary tuberculosis. He began to run down two years ago, and developed chronic laryngitis. When examined, he presented evidence of advanced tubercular disease. The larynx was ulcerated. There was also a lesion on the right tonsil and one on the left. The pain on swallowing was severe. Iodoform gave but little relief. Temporary ease was given by cocaine. An application of lactic acid was made to the right tonsil, and two days later it was much improved. This was repeated and the improvement continued for two weeks, when the patient began to run down and died. The bacillus tuberculosis was not found, but the man had evident pulmonary tuberculosis.

The third case was that of a man, aged 43. Family history good. No history of specific disease. He was well until ten months ago, when his strength began to fail and cough came on. Two months ago, the throat began to grow sore, and within the last two weeks this has increased. Careful examination of the chest failed to reveal any evidence of disease. Examination of the fauces showed ulceration extending out along the upper border of the uvula. Later the signs of pulmonary involvement became evident. Applications of lactic acid gave only temporary benefit. In this case, the disease has extended along the gum.

Three other cases of somewhat similar character were reported.

Dr. E. L. Shurley, of Detroit: In these cases of so-called primary tuberculosis mistakes of diagnosis are sometimes made. He is not sure that the bacillus can be regarded as an absolute evidence that the disease is tubercle in one case, which he consid-

ered to be a case of tubercle of the larynx, and in which the bacillus tuberculosis was found, the patient recovered and is still living five or six years later.

DR. J. O. ROE, of Rochester: I have had two well-marked cases of primary tubercle of the pharynx. The first case was that of a lady 35 years of age. Six months before coming under observation she suffered great pain in the throat. A well-marked lesion was found above the border of the epiglottis and at the base of the tongue. This improved under local treatment. Subsequently the ulceration returned, the patient ran down in health and died. The larynx was entirely free from ulceration. The second case was a gentleman 40 years of age, who had tuberculosis ulceration of the larynx developed. Under the persistent application of iodide of amyl continued for some time the patient recovered, and is still well, weighing 225 pounds.

FRIDAY, MAY 28. SECOND DAY. MORNING SESSION.

At 10 o'clock the executive session was held, and the following business was transacted:

The following were elected

OFFICERS FOR THE ENSUING YEAR

President—Dr. E. Fletcher Ingals, Chicago.

1st Vice-President—Dr. E. Carroll Morgan, Washington.

2d Vice-President—Dr. J. N. Mackenzie, Baltimore.

Secretary and Treasurer—Dr. D. Bryson, Delavan, New York.

Additional Member of the Council—Dr. F. H. Hooper, Boston.

The following were elected

ACTIVE MEMBERS:

Dr. Benjamin F. Westbrook, of Brooklyn; Dr. Frank Donaldson, Jr., Baltimore; Dr. Alexander W. MacCoy, Philadelphia; and Dr. J. C. Mulhall, St. Louis.

Prof. Ramon de la Sota y Lastra, of Seville, Spain, was elected a *Corresponding Fellow.*

A proposition in reference to the formation of a

CONGRESS OF AMERICAN PHYSICIANS AND SURGEONS

was received. The proposition was approved, and the following committee, taken from the ex-Presidents of the Society, was appointed to confer with committees from other Societies: Dr. J. Solis Cohen, Philadelphia; Dr. G. M. Lefferts, New York; Dr. F. I. Knight, Boston; Dr. F. H. Bosworth, New York; and Dr. E. L. Shurley, Detroit.

It was decided to hold the next meeting in New York, the time to be determined by the Council.

DR. WILLIAM H. DALY, of Pittsburgh, read a paper on

THE SIMPLEST AND MOST EFFICIENT TREATMENT OF DIPHTHERIA.

His object was to describe the calomel treatment. This is an old treatment. It is mentioned in the writings of Rev. Sydney Smith. About 1779 Sydney Smith's daughter, when six months old, was taken sick with a violent attack of croup. Dr. Hamilton, of Edinburgh, was sent for, but not being able to come sent directions to "give the child two grains of calomel every hour, and to persevere. He had never known this to fail." This was done and the child recovered.

The credit of practicing and recommending this treatment in modern times belongs to Dr. William C. Ritter, of Pittsburg. Of the different forms of mercury, calomel is the best. The drug is to be given in doses of two to five grains every, one, two, or three hours. It may be given dry and washed down with a little ice water, or it may be given in water. This is to be continued until the stools become frequent and contain in them gelatinous masses of a bright green color, resembling chopped spinach. Then the interval is to be lengthened and the drug continued so as to keep up this condition of catharsis, the patient having two or three stools each day. It is better to lengthen the interval than to diminish the dose. In this way there is less liability to induce ptyalism. Very little depression follows these large and frequently repeated doses. Ptyalism does not often occur. Under this treatment the membrane exfoliates and re-forms, if at all, with less readiness. The fever diminishes. The diet is to be of a light, nutritious character.

DR. E. L. SHURLEY, of Detroit, agreed that the internal administration of calomel is one of the most efficient remedies for croupous inflammation, but in regard to its efficiency in the treatment of diphtheria, he could not agree. The difference between croupous inflammation and diphtheria is considerable. He knew of no cases of diphtheria relieved by the calomel treatment.

DR. F. DONALDSON, of Baltimore, wished that Dr. Daly had given us some statistics and reported some cases. He had seen the calomel treatment frequently tried, but without success. He saw Trousseau treat cases of diphtheria in that way, but he finally abandoned it.

DR. BEVERLY ROBINSON, of New York, said there was scarcely a remedy which had not, at some time, been recommended as a specific for diphtheria. Calomel has been thoroughly used, and finally given up because it did not accomplish what was proposed. He was convinced that epidemics of diphtheria, as of other infectious diseases, differ very much in virulency, and a remedy which is successful in one epidemic will entirely fail in another. He believed that in treatment it is safer to follow what has been considered by the accurate men of our profession, in large numbers, the most reliable treatment for a disease of this character. There is a general consensus of opinion that the proper line of treatment is disinfection of the throat and air passages, with the use of smaller or larger doses of the tincture of iron, combined or not with chlorate of potassium.

DR. F. H. BOSWORTH, of New York, said diphtheria is a blood poison, which often kills by its toxic effect, but in a greater majority of cases it kills by leading to the development of croup. The affection of the larynx is not diphtheria, but a sequent disease. While he thought that mercury had no effect upon diphtheria, he believed that it had a specific effect in

arresting the disease in the larynx called croup. He believes that mercury is the remedy to be administered in croup.

Dr. Daly was not surprised that this method of treatment had not been received with favor. He did not receive it favorably when it was presented to him. He did not recommend calomel as a specific. He simply said that he had a greater proportion of successes under this plan of treatment than under any other which he had tried.

Dr. E. Carroll Morgan, of Washington, read a paper on

THE QUESTION OF HÆMORRHAGE AFTER UVULOTOMY; WITH THE DESCRIPTION OF AN INSTRUMENT FOR ITS ARREST.

The speaker described the following case: A man aged 28 years, whose uvulu had been excised five days previously by another operator, came to him with the statement that the bleeding came on four hours after the operation, and had continued at intervals since then, a large quantity of blood having been lost. Various means had been tried to stop the bleeding, without success. The parts were covered with a coagulum of the persulphate of iron. This was removed and the parts cleansed. The bleeding points were then seen. The uvulu evidently had been greatly hypertrophied. The scissors had been used in the operation. There was no evidence of the hæmorrhagic diathesis in this patient, or in any members of the family. The parts being cleansed, the stump was seized with dressing forceps and the hæmorrhage was controlled. The galvano-cautery was applied and the bleeding stopped. In nine hours it recurred. The cautery was again used and the stump dusted with the persulphate of iron. The following morning a copious hæmorrhage took place, not less than a drachm a minute, by actual measurement, being lost. A careful examination was then made to detect any other source of hæmorrhage, but none could be found. Chromic acid was then used and the hæmorrhage stopped. At three o'clock the same day copious bleeding recommenced. Galvano-cautery was again employed. There was not room for the application of a ligature. Torsion was tried without success. A small clamp used in retaining shirt sleeves in position was then taken, the spring weakened, and the teeth filed down, was then applied. A string was attached to this and brought out of the mouth. This checked the bleeding and was allowed to remain for several hours. It was then removed and the bleeding did not recur. A search of the literature of the subject failed to show any instance in which such a procedure had been before adopted. In this case he thought that this device had saved the patient's life.

A careful study of the subject has enabled him to find seventeen other cases in which the hæmorrhage after this operation had been profuse. Death has never resulted directly from the hæmorrhage after uvulotomy. Where the bleeding is persistent it is always arterial in character.

The speaker, in conclusion, said that a fatal hæmorrhage had never followed uvulotomy. A persistent alarming hæmorrhage is only encountered in the rarest instances. A moderate bleeding, stopping spontaneously or by the use of the mildest applications, occasionally happens. A loss of a few drops of blood at the time of operating, followed by a slight oozing, is a common occurrence.

The most reliable surgical measures for controlling uvular hæmorrhage are, ligature, compression by a clamp or forceps, and the use of the actual cautery. The most reliable styptics are, in the order named, solid nitrate of silver, persulphate of iron, gallic acid, tannic acid, alum, the local use of ice, and vinegar. The most reliable systemic remedies are opium, acetate of lead, sulphuric acid, and ergot. The speaker recommended that in operating, the object should be to restore the uvula as nearly as possible to its normal size, and that the entire uvulu should not be removed.

Afternoon Session.

Dr. Thomas R. French, of Brooklyn, read a paper on

THE LARYNGEAL IMAGE AS SEEN IN PHOTOGRAPHS TAKEN DURING THE PRODUCTION OF TONES IN THE SINGING VOICE.

The speaker had made numerous investigations in this subject, and his observations failed to confirm the statements of most authorities in regard to the position of the vocal cords during the production of different notes in the singing voice. Different individuals evidently use different mechanisms in the production of different tones. The general result of his investigations seemed to indicate that in the production of low notes the vocal cords are separated to a greater extent posteriorly than anteriorly. In the production of the middle tones, the vocal cords are parallel, and in the production of high notes the opening is widest in front. The paper was illustrated by numerous photographs thrown upon the screen.

The following papers were then read by title:

Clinical Notes on Prolapse of the Laryngeal Ventricles; by Dr. Geo. W. Major, of Montreal. *Three Cases of Thyrotomy; Recovery in Each Case, with Good Voice;* by Dr. Clinton Wagner, of New York. *Alarming Hæmorrhage after Tonsillar Excision, Arrested by Torsion of the Artery;* by the same author.

Dr. Wm. C. Jarvis, of New York, described

A NOVEL PROCEDURE FOR THE REMOVAL OF SUB-GLOTTIS LARYNGEAL GROWTHS.

The following case was described:

J. C., aged 35 years, consulted me on March 22, 1882, for the relief of a difficulty of speech and of breathing. The trouble with the voice was noticed two years before; the difficulty in speech appeared one year before coming under observation. The laryngoscope showed a mass of papillomatous tissue occupying the cavity of the larynx. The vocal cords and ventricular bands were not involved. The attempt was made to seize the growth during phonation with the Mackenzie forceps; only a few pieces were removed. The écraseur was then tried, but it was found impossible to use this instrument on ac-

count of the wire being seized by the spasmodic contraction of the vocal bands and displaced. Chromic acid applications were then tried, but although the growth was touched, it was found impossible to limit the application to the diseased structures. The patient then disappeared and did not return for a year. His condition was then much worse, and the difficulty of breathing greatly increased.

It was then determined to try the following plan, and if this failed, to remove the growth through an opening in the wind-pipe. Forceps similar to the Mackenzie instrument, but heavier, was secured. Through the tips of the blades holes were drilled, and through these was passed a piece of No. oo piano wire, which was conducted through a second opening at the angle of the blades and out to the handles, where it terminated in a loop into which the extremity of the index finger could be placed. When the blades were separated this wire formed a cross-bar uniting their extremities. When the blades were closed the loop of wire could be drawn up by the finger. The operation was performed without an anæsthetic. The tongue was depressed and with the aid of the mirror the forceps was introduced into the cavity of the larynx. The mirror was then removed and the forceps carried down to the glottis, through which the tip of the instrument was forced with considerable trouble, on account of the spasm. The instrument was carried into the trachea sufficiently far to be sure that it was below the attachment of the growth. The blades were then separated, and pressing against the anterior wall of the larynx, the instrument was gradually raised until the wire was caught. The blades were then closed and clamped, the loop of wire drawn in and the forceps removed, bringing with it a growth. The laryngoscope showed a second growth, which was removed in the same way. Breathing was at once rendered easy and the speech became natural. The patient said that the presence of the forceps caused no pain, although uncomfortable. This is the only case on record of the removal of a sub-glottis tumor without anæsthetization, and without an opening into the larynx.

DR. T. AMORY DEBLOIS, of Boston, read a paper on

CASES OF LARYNGEAL ŒDEMA.

The author gave brief details of fourteen cases of laryngeal œdema coming under his observation. All of these cases recovered; six under the use of astringents, and eight after scarification.

DR. FRANK DONALDSON suggested that in these cases intubation of the larynx, after the O'Dwyer method, might be applicable.

DR. E. CARROLL MORGAN said his experience with this condition had been limited, and the cases had been mild. He usually employed active purgation, with vesiculation over the larynx and various local measures.

DR. W. C. JARVIS some time ago used, in a case of marked œdema, the fluid extract of jaborandi. In forty-eight hours the œdema had greatly diminished. He had since seen reported several cases in which pilocarpin was used with advantage. He had never seen good results from scarification.

A paper by Dr. Charles H. Knight, of New York, describing *A Case of Perichondritis of the Larynx*, was read by title.

DR. EDGAR HOLDEN, of Newark, reported

A CASE OF GUMMATOUS DISEASE OF THE LARYNX, WITH SPONTANEOUS RE-OPENING OF THE LARYNX AFTER THYROID LARYNGOTOMY.

L. D., aged 35, presented himself, May 20, 1885, with dyspnœa and aphonia. Examination showed a cicatrix on the epiglottis, and the left side of the larynx presented a rounded mass of dull appearance. He denied syphilitic infection. After a careful consideration of the case, the probabilities were thought to favor syphilis, and he was given iodide of potassium in large doses. There was improvement for a short time, but he soon became intolerant to the drug. Thyroid laryngotomy was considered necessary and was performed. The tumor was removed with the écraseur; no ulceration was visible. Tracheotomy was then performed, and the upper incision closed with sutures. The patient did well and the tube was removed on the tenth day. The external wound healed. He insisted on returning to his home, where the surroundings were very unfavorable and his health deteriorated. On the fortieth day after the healing of the wound, the laryngeal incision was torn open by a severe cough. He refused to return to the hospital. Examination of the larynx showed no disease. Under the surroundings it was impossible to get the wound to heal. His health continued to run down, and six months and fourteen days after the operation the patient died of exhaustion. The patient had lived five months with an opening in the larynx.

DR. U. G. HITCHCOCK, of New York, read a paper on

GUMMATOUS INFILTRATION OF THE BASE OF THE TONGUE,

giving the details of four cases coming under his observation. In none of these cases was the fibrous septum the exclusive seat of the disease. In three cases, the deposit did not approach this structure. In these cases the degree of dysphagia was in direct proportion to the proximity of the deposit to the base of the tongue.

SATURDAY, MAY 29, THIRD DAY—MORNING SESSION.

A paper on *A Case of Hysterical Sneezing, Apparently Cured by Applications to the Nasal Passages of the Continuous Battery Current*, by DR. S. SOLIS COHEN, of Philadelphia, was read by title.

DR. JOHN N. MACKENZIE, of Baltimore, read

A CONTRIBUTION TO THE PATHOLOGY AND TREATMENT OF THE RESPIRATORY VASO-MOTOR NEUROSES.

The naso-bronchial tract is frequently the seat of periodical vascular disturbance in which explosions of nerve force play a conspicuous part. These probably depend upon some form of sympathetic or vaso-motor irritation. In the production of such conditions two elements enter: a depraved state of the nerve centres and an abnormal excitability of certain

portions of the naso-bronchial tract. In regard to the hyperæsthetic condition of the nasal mucous membrane, the hyperæsthesia characteristic of the paroxysm is like the vaso-motor phenomena which accompany it, purely secondary, and occurs as a result of central irritation or paresis. This may pass away with the subsidence of the other symptoms or be more or less constant. But in either case, there are certain areas in which the hyperæsthesia is more pronounced. The lower, posterior parts are the portions of the tracts usually affected. In hay fever we are dealing with a neurosis. In this connection, he called attention to a neurosis of the aural cavity which was closely allied to vaso-motor coryza and also to a similar condition of the conjunctiva. These cases are a further evidence of the sympathetic nature of these affections. With reference to asthma in connection with nasal disease it was stated that the ancients had frequently noted the expulsion of mucus from the nostril accompanying or following an attack of asthma. The older writers were evidently familiar with hay fever, but they confounded it with the bronchial asthma of the present day. Asthma is looked upon as a disease *per se*, but, like many other disturbances of respiration, it has no particular lesion and is common to many pathological states. It, like cough, is most commonly symptomatic of some disorder of the respiratory tract. An interesting feature in a certain proportion of cases is the association of urticaria with coryza. This is a familiar observation. The speaker then considered the relations between the skin and the respiratory tract. He regarded the skin as having a function similar to that of the lungs, on the principles which he explained he based the treatment of hay fever, or periodical vaso-motor neurosis of the respiratory tract. From sixty to eighty cases had been treated after this plan. In the greater number of cases the nasal passages were the seat of the vascular disturbance. Next in frequency came the pharynx. Occasionally it was localized in the laryngeal cavity. In whatever portion the vaso motor neurosis is situated, the general principles are the same. The treatment of one of these affections is essentially the treatment of the others. Hay fever and asthma may be classed as symptoms owing their origin to the same cause.

In the treatment of hay fever the chief indications are to remove any existing local respiratory disease, to so alter the nutrition of the nerve centres that they may not respond so easily to irritation, and to carefully search for pathological conditions and adopt appropriate treatment for their removal when found.

These chronic neuroses require chronic treatment. The peculiarity of the sympathetic is with the patient all the time, by night and by day, and at any moment may give evidence of its presence by a paroxysm. Any treatment which is practised simply during the attack or immediately before is simply palliative. In reference to the use of cocaine, he had found that it increased the irritability of these structures, and its application to the erectile tissue may ultimately weaken the cell walls. He, however, did not question the excellent virtues of this remedy in certain acute affections. In hay fever he had failed to find improvement after its use. For a short time amelioration was produced, but in the course of an hour the symptoms recurred. His custom is to treat hay fever as any other chronic disease of the nervous system. He insisted upon a prolonged tonic course of treatment during the intervals. This may at first fail, but if persisted in, the time will come when, if there is no incurable lesion, the paroxysms will diminish in severity and frequency. The great mistake is made in the suspension of the treatment upon the termination of the attack. Among the remedies for the constitutional management of this class of cases he recommended phosphide of zinc, quinine and nux vomica. These may be given in combination in the following proportions:

℞ Zinci phosphide..... gr. 1-16.
Quiniæ sulphat..... gr. 2.
Ext. nucis vomicæ..... gr. ¼.
M. Et ft. pil No. 1.
Sig. To be taken before meals.

After meals he directs that from 3 to 5 drops of liquor arsenici et hydrargeri iodidi be taken in a wine glassful of water. A similar line of treatment had been employed for several years by the speaker in simple inflammatory conditions of the nose and throat with good results. He had also employed bromide of potassium with advantage. A constant current of from ten to fifteen cells, with one pole over the nape of the neck and the other over the region of the superior cervical ganglia or in the nasal passage has in a few cases been beneficial. In one case improvement followed the obliteration of vessels on the posterior wall of the pharynx.

In regard to topical applications to the existing nasal conditions, these accomplish one result only. They close the door to irritation of the nerve centres. In many cases this is of itself sufficient. There are other cases in which such a course will fail. In these neuroses, we cannot expect to thoroughly eradicate the disease until the sympathetic nerve irritation is overcome.

Dr. William H. Daly: The iodide of sodium has given him better results than any other single remedy. He gives it in small doses long continued. The upper air passages should be carefully examined, and upon the slightest suspicion of local disease, the condition should be remedied. If this is done, he believes that the disappointments will be few. The constitutional treatment should, he thinks, be secondary to the local treatment.

Dr. E. Carroll Morgan: The galvano-cautery has been highly recommended as a successful remedy for hay fever, but his experience with it has not been so satisfactory as that of some other operators. In considering these cases of hay fever, he has been struck with the apparent immunity from this affection experienced by those suffering with anosmia. He has never seen essential anosmia in a subject of hay fever. He would ask the experience of the Fellows in regard to the efficacy of the galvano-cautery treatment as a preventative or a cure for hay fever.

Dr. C. E. Sajous, of Philadelphia: In the early part of his experience with the galvano-cautery, he had a number of favorable cases. Last year he met

with a number of unsatisfactory cases. What it depended on he does not know, but the results were not by any means as good as those of the preceding year. It certainly was not from want of attention on his part. Although all the cases were benefited, he thinks he did not obtain absolute relief in more than 45 per cent. Some of the cases treated two years ago, and which escaped the attack the following year, had a recurrence last year. This is a matter of great importance, because in his work, he advanced the opinion that these cases were absolutely cured. Some of the cases have entirely escaped. One case treated in 1881 with glacial acetic acid has had no recurrence.

DR. CARL SEILER, of Philadelphia: The failures of the treatment of hay fever may, he thinks, be explained by the supposition thrown out in the paper read. The Schneiderian membrane is not the only source of irritation of the vaso-motor nerves. The source of irritation may be situated in the pharynx or in the larynx. If the case does not yield to treatment of the nasal membrane we should look further. Two years ago a case of hay fever presented itself to him. Glacial acetic acid was applied, and the following summer he was free from hay fever, but suffered with asthma. Examination showed a large posterior hypertrophy; this was removed, and since then he has had neither hay fever or asthma.

DR. SAMUEL JOHNSTON, of Baltimore, read a paper entitled

A CASE OF NASO-PHARYNGEAL GROWTH.

A child was brought to him on account of difficulty in breathing through the nostril. Examination showed that there was a tumor filling up the posterior part of the right nostril. After a short preliminary treatment, the attempt was made to remove the growth with the écraseur. A spray of a 4 per cent. solution of cocaine was employed. A cord was first passed through the nostril and brought out of the mouth, so that if necessary, the nostril could be plugged without delay. The wire of the écraseur was applied without difficulty. When the growth had been cut through about two-thirds, the shaft of the instrument broke, leaving the wire and about three-fourths of an inch of the instrument attached to the growth. An attempt was made to apply a second écraseur, but this failed. After trying to remove the portion of instrument broken off, it was decided to wait a short time and allow the growth to slough off. Four days later the attempt to apply an écraseur was again made and succeeded without difficulty. The tumor measured one and one-half inches in diameter and was fibroid in character.

DR. C. C. RICE, of New York, read a paper entitled

WHAT CASES OF NASAL CATARRH REQUIRE SURGICAL TREATMENT?

Almost every nasal chamber will exhibit irregularities and abnormalities of some kind. The introduction of more improved methods of determining the locality of inflammation has led to a more frequent resort to surgical measures. With the galvano-cautery almost any effect, from a slight stimulation to destruction of tissue, can be obtained. Those who condemn this instrument should state what use of it they object to. Not every case of anterior hypertrophy should be operated upon. The erectile tissue in this situation serves a valuable physiological function in swelling up and excluding irritating particles from the lungs. In determining whether or not an operation is required, the sensations of the patient should be taken into consideration, with the results of the examination. If after the use of a four per cent. solution of cocaine, sufficient hypertrophy to interfere with breathing is still apparent, it should be removed.

DR. F. H. BOSWORTH said the speaker had referred to the erectile tissue of the nose as possessing a physiological function. He considered that this tissue is not a true erectile tissue. It assumes the erect position only as a morbid condition. It is simply an enlarged conglomerate mass of blood vessels. There is no physiological function observed by its swelling up. The speaker also states that all persons will present abnormalities of the nose. It is a mistake to suppose that we have morbid conditions in every nose. The healthy nasal cavities present a typical appearance.

DR. J. N. MACKENZIE said the microscope shows that the erectile tissue of the nose corresponds with the erectile tissue in other portions of the body. He believed that these bodies serve the physiological purpose of excluding irritating bodies from the lower respiratory tract. Experiments with horses driven through a cloud of dust have shown that while the anterior portion of the nose was filled with dust, none passed into the larynx.

THE PRESIDENT thought the question of the erectile tissue of the nose should be treated on a broader basis. In man the nasal chambers are exceedingly degenerate. There is no animal in which the nose is so small in comparison with the rest of the face, as in man. It is well known that structures which are passing through a process of degeneration are exceedingly variable. So it is with the human nasal chambers. Remembering this fact, we come to another of great interest, that all animals with short faces, as the rabbit and cat, have on the nasal septum, erectile bodies. He had always held that the object of these bodies was obstructive, and that by swelling up they protected the portions behind. If that is the case, he could not see why we cannot claim that erectile tissue exists in man. It is his opinion that erectile tissue does exist, and is protective and obstructive.

The following papers were read by title: *Inflammation of the Antrum;* by Dr. Beverly Robinson, of New York. *Additional Notes on a Case of Erysipelas of the Larynx;* by Dr. Wm. Porter, of St. Louis.

A vote of thanks was then tendered the Philadelphia College of Physicians, the Union League Club, the University Club, and the managers of the Pennsylvania Hospital, for courtesies received, and the Association then adjourned.

DOMESTIC CORRESPONDENCE

LETTER FROM NEW YORK.

(FROM OUR OWN CORRESPONDENT.)

The Future Influence of Surgery and Surgical Resources upon Obstetrics—Amendment to the Constitution of the Academy—A Pasteur Institute.

At the meeting of the Academy of Medicine held on June 3, Dr. C. C. Lee, one of the surgeons to the Woman's Hospital, read a paper on "The Future Influence of Surgery and Surgical Resources upon Obstetrics." It was, to be sure, almost exclusively devoted to the past, but the idea was, that since surgery had already had such a marked effect upon midwifery, it was reasonable to suppose that still greater triumphs were in store for it in this department. There was nothing, he said at the outset, more impressive to those interested in the subject of obstetrical procedure than the rapid inroads made within the last four years by surgery into a domain which had been hitherto regarded as one purely medical.

He first took a glance at the history of the art of midwifery from the earliest times, giving the ancient Egyptians full credit for the very considerable progress which they made in it, and then, gradually coming down to more recent periods, he took up the subject of the Cæsarean section and gave an account of its history, its dangers, and the method of its performance; after which he spoke of the four modifications of the operation suggested respectively by Francke, of Cologne, Sänger, of Leipsic, and Kehrer and Cohnstein, of Heidelberg. Kehrer's method is described as consisting of a transverse incision through the anterior convex surface of the uterus at the line of the *os internum*, of deep sutures to secure immediate contact of the peritoneal borders, and of strict antiseptic precautions during the operation, combined with subsequent drainage of the abdominal cavity. In order to cover the line of incision with peritoneum he directs that the latter be separated from the underlying muscle for nearly a half inch from the borders of the wound. Dr. Lee gave a detailed description of this procedure, as well as of Sänger's "subperitoneal muscular resection," and its modification by Leopold, and of the method proposed by Cohnstein, to turn the uterus completely out of the abdomen, and open it from behind, which, so far as he knows, has never as yet been attempted in actual practice.

In order to show that the great danger in connection with the Cæsarean section arises from the delay commonly made in resorting to the operation, he quoted the opinion of Dr. Robert Harris, of Philadelphia, whom he pronounced not only the highest authority on Cæsarean section, but the greatest medical statistician that this country has ever produced. He then went on to speak of the substitutes which had been devised to take the place of this operation; first describing Porro's operation, which involves the removal, after the performance of the Cæsarean section, of both uterus and ovaries, and its modification by Müller, who recommends that the uterus, after the enlargement of the abdominal wound, shall be tilted laterally, and withdrawn outside the abdomen previous to section.

Dr. Lee spoke in high terms of the other substitute for Cæsarean section, laparo-elytrotomy, as suggested and successfully practised by Dr. T. Gaillard Thomas. Although this procedure was proposed as long ago as 1806 by Jörg, who, however, contemplated cutting through the peritoneum, and again in 1820, by Ritgen, and in 1822 by Physick, of Philadelphia, Dr. Thomas was unaware of these facts at the time he worked out the problem for himself by a number of operations on the cadaver. Dr. Lee thought that he was fairly entitled to the credit of the operation, and that if he had accomplished nothing else, this achievement alone would serve to hand down his name to posterity as one of the great masters of obstetric surgery. By this method the dangers of opening into the perineum and wounding the uterus are avoided by an incision of the abdominal parietes along the upper edge of Poupart's ligament, lifting the peritoneum, dissecting down to the vagina, dividing the latter transversely, and then, having reached the cervix, extracting the child through the passage thus artificially created. Although the number of cases in which laparo-elytrotomy had been performed was as yet comparatively small, the proportion of successes thus far had been much greater than had been met with either in Cæsarean section or the Porro or Porro–Müller operation. During his subject, Dr. Lee quoted a number of statistics, which were almost entirely taken from Dr. Harris's recent article in the International Encyclopædia of Surgery.

He next took up the matter of extra-uterine pregnancy, and showed what excellent results had been accomplished by the use of electricity in the earlier months, as first successfully employed by Dr. J. G. Allen, of Philadelphia. Having spoken also of the results obtained by elytrotomy and laparotomy in the later months of ectopic gestation, he went on to treat of lacerations of the cervix uteri, and condemned the primary operation in such cases. Personally, he said, he had never been successful in securing union by this procedure, and he knew of but one surgeon who claimed to get good results from it. In a large number of cases which are left to nature no subsequent operation was required.

With regard to lacerations of the perineum, however, the case was very different. Here the advantages of immediate closure were so immense that he regarded it as a matter of great importance, and he said that he would now as soon think of leaving a parturient with an undelivered child in the uterus as to neglect to sew up a torn perineum. The omission to do this was the source of much suffering and misery which might otherwise have been avoided. Notwithstanding the fact that operative surgery had already accomplished so much in the field of obstetrics, he felt confident that, judging from the triumphs of the last few years, the results noted would be still more satisfactory in the future.

Dr. Wm. M. Polk remarked that for a considerable period after he commenced the practice of medicine, he ranked himself as strictly a physician. In the course of time he became an obstetrician; but he

entered upon his work in this department with the conviction that it was simply a branch of the physician's art. This feeling was to a great extent due to the training which he had received, and especially to the teachings of Professor Fordyce Barker. After a short time, however, he began to appreciate the fact that there were certain points in obstetric practice involving surgical methods, which if, at the present stage of scientific progress in the profession, a man were to neglect, he would soon be driven to the wall. It was only recently, indeed, that obstetricians had become alive to the fact that the domain of their art was in reality one of vast scope. In the English hospitals, more than in this country, they have had to fight their way surgically against the mere physicians, and also to contend against the active opposition of those recognized as general surgeons. But the same had been true to a considerable extent here also, and he was happy to say that the obstetrician, notwithstanding the indifference manifested on the one hand, and the active opposition on the other, had now won for himself an enviable position. By way of illustration he need only point to the matter of abdominal section.

At one time vesico-vaginal fistula was almost as much a reproach to the profession as cancer; but at present large numbers of women were perfectly relieved who formerly would have been left to drag out lives of misery and suffering. But the pendulum, once started, was apt to swing a little too far, and it was perhaps true that to-day the obstetric surgeon was not infrequently a little too free with the use of his knife. As to lacerations of the cervix resulting from parturition, he had seen many serious cases (the rent sometimes extending to the vaginal roof), recover perfectly under ordinary treatment, without resorting to operative measures; and his views on this point were quite in accord with those expressed by Dr. Lee. Surgeons complained that the abdominal cavity was opened too often at the present day. It might be; but that was simply a side issue. Before the obstetricians of to-day, Dr. Polk said in conclusion, lay the grandest problem that could possibly engage their attention, viz.: that in connection with the Cæsarean section. He did not doubt that within the next ten years it would be possible to entirely eliminate from practice that most repulsive of all obstetrical procedures, the destruction of the life of the fœtus by craniotomy. When this had been accomplished there would have been achieved the greatest triumph ever won in surgery; and the honor of it would belong to the obstetrician.

Dr. Fordyce Barker, in making some remarks on the primary operation in laceration of the perineum, said that he was not inclined to accept any new proposition until time and experience had determined its real merits; and for a long time he had held his opinion on this subject in abeyance. Some years ago, at a meeting of the American Gynecological Society, a distinguished obstetrical authority had made the statement that any physician who did not close by primary operation a rupture of the perineum occurring in his practice was guilty of a neglect of duty. He did not express any positive opinion in regard to the matter at that time; but he hesitated to accept this dictum as correct, and at the present time he was still less inclined to acquiesce in its sentiment. He then made the announcement that in no case that he had ever attended in his private practice had a secondary operation been performed; and this was simply for the reason that it had never been required. He did not mean to say that he had never had any perineal lacerations; but only that none of them are of such a character as to demand a secondary operation. The uterus retained its position perfectly in every instance, and no other bad result occurred from the rupture. He could say, furthermore, that up to the past winter, in no private case of his had a primary operation ever been performed.

Dr. Barker then referred to a peculiar epidemic which had occurred in this city during the six months ending May 1. There were no unfavorable symptoms until about the sixth day after labor, when an explosion would suddenly occur in the system; the temperature rapidly running up to 104, 105 or 106°. In consultation he had seen forty such cases, but only one case had occurred in his own practice. As a rule, the symptoms all disappeared within three days, and out of the forty he was called to see only twenty more than once. There was, however, one fatal case. It was a significant fact, he thought, that in five out of the forty cases the primary operation for lacerated perineum had been performed; but in only one of them did union take place. In the case occurring in his own practice there was a bad laceration, as the labor was a very difficult one; but the result of the primary operation was unsuccessful.

In two cases that he knew of, in which immediate closure of a ruptured perineum was effected, there was profuse uterine hæmorrhage, as a result of shock. After referring to other cases, he said that his mature opinion was, that while there were, no doubt, cases in which the condition of the patient was so favorable that primary union was likely to result from an immediate operation, yet as a rule it was safer to avoid the operation, and trust to absolute cleanliness and watchful care of the patient. If there was any tendency to septic trouble, he believed that the sewing up of the perineum would be likely to be attended with unpleasant consequences on account of the interference which was thus caused to free drainage.

Dr. Barker then went on to speak of the marked influence which the gynecologist has had upon the surgery of the present time, and in this connection referred to the last paper which Dr. Marion Sims had read before the Academy, in advocacy of laparotomy in traumatic lesions of the abdomen; when the late Dr. James R. Wood, in a forcible address, had argued against this procedure, on the ground that while the gynecologist could carefully prepare his patient for the operation, the general surgeon would be called upon to operate suddenly, and under the most disadvantageous circumstances.

Dr. Malcolm McLean said that he felt compelled to advocate the desirability of the primary operation in lacerations of the perineum. He had found that in those cases in which he sewed up the laceration with soft silk sutures, the result was more satisfactory

than in those which were left to nature. He did not believe in using silver wire sutures, as they were apt to displace the parts; but silk sutures, deeply and carefully placed, were generally efficient and satisfactory, while they had the advantage, at all events, of not doing any mischief, even if they did not accomplish the desired result. However, this procedure served a good purpose by its moral effect, in backing up the accoucheur in case union did not take place; for in these days when so much was expected of one from a surgical point of view, a reflection was apt to be cast upon the medical attendant if there resulted a permanent laceration in which there had been no attempt made to secure union by operative measures. In the main, however, he had found this simple operation of decided practical advantage.

Dr. Polk then stated that he entirely acquiesced in the opinion expressed by Dr. McLean. He would admit, however, that there were exceptions, in which the conditions described by Dr. Barker contraindicated the primary operation. There were many cases in which the accoucheur had had a bad operation, or labor had been unduly prolonged, where the existing condition of shock would be likely to be followed by hæmorrhage; and here we are justified in declining to operate. We should always be sure that the patient had not already been subjected to the risk of shock before we made any attempt to repair the laceration. It was now some time since he had abandoned the use of wire sutures in either primary or the secondary operation. It was natural that the latter should have been preferred to all others before the days of antiseptic surgery; but silk properly prepared, and soaked in a bichloride solution, was altogether better than the metallic suture, which was always a cause of great pain and annoyance to the patient.

In closing the discussion, Dr. Lee said that he differed from Dr. Barker in regard to the question of primary operation in perineal lacerations. It might be that he had not been able to place his patients under the same favorable conditions for spontaneous recovery which the latter had; but, at all events, he had not been fortunate enough to obtain the same satisfactory results when such cases were left to nature. He then referred to the practice observed in the lying-in Department of the New York Foundling Asylum, in which the patients were nearly all primaparæ. In this institution it was the uniform rule in every case, whether the patient was attended by the house-surgeon or one of the visiting physicians, that immediately after labor the condition of the perineum should be carefully ascertained, and if it was found to be decidedly torn, to close the laceration at once. Of course he did not wish to be understood as advocating an operation when there was merely an abrasion, or a tearing of the fourchette, such as was almost universally the case with primaparæ. He did not believe, either, in operating when there was profound shock present from loss of blood, or other cause; but he had very rarely met with such a condition in his practice.

The plan which he adopted was simply to continue the chloroform under the moderate use of which the child had been born, and to make an examination of the perineum; when, if a decided rupture was found, the chloroform was still kept up, and the laceration repaired by means of silk sutures. Like Dr. Polk, he had not in some time employed silver wire sutures in either primary or secondary operations. If the posterior wall of the vagina was involved in laceration, he usually put in two or three catgut sutures at this point. The whole operation was a very simple one, but he always performed it under chloroform, and he had never seen shock caused by it. In his practice at the Woman's Hospital he had seen so many bad results, which might have been avoided, which were the direct consequence of ruptures of the perineum which were allowed to go without operative interference at the time of their occurrence, that he was firmly convinced as to the great utility of the primary operation.

At this meeting the Academy passed the amendment to its constitution which has before been referred to, which gives it once more the undisputable right to discipline its Fellows "for cause."

A meeting was recently held at the house of Dr. Alexander B. Mott, for the purpose of organizing a Pasteur institute in this city, for the treatment of persons bitten by rabid dogs. It was estimated that about $5000 would be required for the first year's expenses, and it was accordingly decided to issue an appeal to the public for subscriptions, as it is proposed to have the inoculations entirely free. Dr. Valentine Mott has recently returned to this city from Paris with a rabbit inoculated by Pasteur, and it is stated that this is the first time that the latter has permitted his virus to go out of his own hands; which certainly is very complimentary to Dr. Mott. It would seem that this movement is a little premature at the present time; but if the success of Pasteur's process is finally demonstrated, of course it is only a question of time when such institutes will be established in many large cities in various parts of the world. The officers of the proposed institute are as follows: President, Dr. Alexander B. Mott; Vice-Presidents, Drs. Alexander F. Liantard and Louis Deplasse; Recording Secretary, Dr. Charles Villa; Corresponding Secretary, Dr. Valentine Mott.

P. B. P.

ILLINOIS COLLEGE OF PHARMACY.

To the Editor of the Journal:

Dear Sir:—Being a strong advocate of advanced medical education, and hence by necessity of properly educated pharmacists, it is with a feeling of regret that in the May 22 number of The Journal, I notice, in an announcement of a new pharmaceutical college, what is, in its essence, an attack on the best interests of pharmacy, and hence of medicine, in Illinois. It was said by Prof. Oldberg, (*Pharmacist*, June, 1885) that, "it requires a very considerable outlay of money and large classes to maintain an efficient college of pharmacy, and the multiplication of schools without visible means of support is to be condemned. There are already more colleges and schools of pharmacy, than there is any demand for,

and each new addition is a positive injury to the cause of sound education."

Judged by this standard the establishment of a new college of pharmacy, on the grounds alleged in the editorial, was indefensible. The only grounds on which the establishment of a new college of pharmacy could be justified from an ethical standpoint, would be that the old college was lowering its standard of graduation and education, whereas the opposite was the case, and was demonstrably the cause of the secession which led to the establishment of the new college. None of the gentlemen mentioned as the prospective faculty can be considered pharmacists. In the establishment of a college of pharmacy the scientific and not the commercial aspect deserves the first consideration. In creating a new college of pharmacy Prof. Oldberg and his colleagues have (*to use his own words*), therefore, positively injured the cause of sound pharmaceutical education.

Very truly,

FRED. M. SCHMIDT, Ph.G.

Secretary Chicago College of Pharmacy.

DANGERS OF KISSING.

TO THE EDITOR OF THE JOURNAL:

Dear Sir:—The paper recently published in your journal on "The Dangers of Kissing" has apparently attracted the attention of the medical profession. Private communications and clippings have encouraged me in an attempt to pursue the study further. Consequently, I hereby request the reports of cases of disease having been communicated by kissing, for use in another paper. Very truly, etc.,

SAMUEL S. ADAMS, M.D.

1525 I Street, Washington, D.C.

BOOK REVIEWS.

DIAGNOSIS AND SURGICAL TREATMENT OF ABDOMINAL TUMORS. By SIR SPENCER WELLS, BART., etc. Sm. 8vo., pp. 216. Illustrated. Philadelphia: P. Blakiston, Son & Co. 1885. Chicago: W. T. Keener.

Though this volume might be called a fourth edition of a book published in 1865, it is necessarily in very many respects a new work. In scarcely any department of medicine have two decades made more marked changes than in the treatment of abdominal tumors. Twenty-five years ago when a physician undertook the treatment of an abdominal tumor he began a journey in the dark. Ovariotomy sometimes succeeded, in which case the surgeon was considered lucky; but it as often failed, and was very generally discredited.

The first edition of this book might have been called a plea for ovariotomy; to-day it is no longer necessary to enter any plea at all for the operation. After the publication of the second, in 1872, Mr. Spencer Wells thought that with a record of 500 cases he might with propriety go into the diagnosis of ovarian disease and the details of ovariotomy. The third book, in 1882, included uterine pathology and surgery, which had been grafted upon that of the ovary. The present book goes still farther, and presents the most advanced knowledge of one of the greatest of specialists. It is divided into two parts; the first devoted to "Ovarian and Allied Tumors," and the second to "Uterine and other Abdominal Tumors." Every one knows who Sir Spencer Wells is, and what he has done. The volume under consideration is condensed and clearly written, and has a good index.

MISCELLANEOUS.

THE BOWDOIN MEDICAL ALUMNI ASSOCIATION will hold its third annual meeting in Memorial Hall, Brunswick, at 12 M., June 23.

ELECTROLYSIS FOR STRICTURE OF THE URETHRA. It is said that a special department for the treatment of stricture by electrolysis is now being organized at St. Peter's Hospital for Urinary Diseases, in London.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JUNE 5, 1886, TO JUNE 11, 1886.

Col. J. H. Baxter, Chief Medical Purveyor, ordered to proceed to New York City on public business, and on the completion thereof to return to his station. (S. O. 128, A. G. O., June 3, 1886).

Lieut.-Col. Charles Page, Surgeon, granted leave of absence for one month, with permission to apply for ten days' extension. (S. O. 55, Dept. Mo., June 1, 1886.)

Major Blencowe E. Fryer, Surgeon, sick leave of absence still further extended six months on surgeon's certificate of disability. (S. O. 131, A. G. O., June 7, 1886.)

Major Henry R. Tilton, Surgeon, ordered for duty as Post Surgeon, Presidio of San Francisco, Cal. (S. O. 38, Dept. Cal., June 1, 1886.)

Capt. G. W. Adair, Asst. Surgeon, granted leave of absence for two months, to take effect when his services can be spared. (S. O. 128, A. G. O., June 3, 1886.)

Capt. W. F. Carter, Asst. Surgeon, ordered for duty (temporary) at Ft. Concho, Texas.

Par. 2, S. O. 55, Dept. Texas, c. s., granting Asst. Surgeon Carter leave of absence for one month, is revoked. (S. O. 64, Dept. Texas, June 1, 1886.)

First Lieut. B. S. Black, Asst. Surgeon, ordered for duty at Ft. Stockton, Tex. (S. O. 64, Dept. Texas, June 1, 1886).

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE HOSPITAL SERVICE FOR THE THREE WEEKS ENDED JUNE 14, 1886.

Mead, F. W., P. A. Surgeon, granted leave of absence for twenty days. June 9, 1886.

Guitéras, John, P. A. Surgeon, granted leave of absence for thirty days. June 14, 1886.

Watkins, R. B., Asst. Surgeon, granted leave of absence for thirty days. June 4, 1886.

Pettus, W. J., Asst. Surgeon, to proceed to Charleston, S. C., for temporary duty. June 11, 1886.

CORRIGENDA.

In THE JOURNAL for May 29, p. 605, the name of C. W. Day is given as the member of the committee on State Medicine from Louisiana. It should have been Richard H. Day, of Baton Rouge, La. In the same journal, on page 600, the title of Dr. A. R. Robinson is given as "Lecturer," when it should have been Professor of Dermatology and Syphilis.